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ART. XXVIII.—*On the Examination of the Bessemer Flame with Colored Glasses and with the Spectroscope;* by J. M. SILLIMAN, M.E., Adj. Prof. of Metallurgy, Lafayette College, Easton, Pa.

[Read at the Troy Meeting of the Am. Association for the Advancement of Science.]

I. *Examination with Colored Glasses.*

IN the Bessemer process, the progress of the decarbonization is determined chiefly by the appearance of the smoke, flame and sparks which are emitted from the apparatus. Owing to the rapidity with which the reactions take place, it is highly important to catch the exact moment when the blast should be turned off. This is indicated by the color and brightness of the stream of gas issuing from the converter, and by this the moment of total decarbonization can generally be accurately determined by the naked eye. When, however, pig-iron of certain qualities is used (manganiferous iron, for example) this determination is very difficult; even those who have had much experience make frequent mistakes and find it impossible to produce the same quality of steel at every blow.

In order to intensify these flame-indications, use has been made of the spectroscope, and also of various combinations of colored glasses. The former was first attempted by Dr. Roscoe, and the latter by Mr. Rowan at the Atlas Works.

Mr. Rowan experimented with a great variety of colored glasses and obtained the best results by using three glasses, two of ultramarine blue and one of dark yellow. This little instrument, or chromopyrometer, as he terms it, is now in daily use at the Atlas Works, its indications being so marked and unmistakable as to render its use safe in the most inexperienced hands.

The following experiments were made at the Bessemer Steel Works of John A. Griswold & Co., in Troy, while pursuing the chemical course in the Winslow Laboratory of the Rensselaer Polytechnic Institute. In my observations on the flame I made use of the spectroscope, and also of a combination of colored glasses. This combination consisted of two light-yellow glasses and a blue one, through which the sunlight appeared of a deep purplish-blue tint; and as it differed slightly from Rowan's, it gave somewhat different results.

In order to reproduce the appearance of the flame at the different stages of the process, I prepared a plate consisting of about a hundred varieties of colors and tints, all of which were numbered and thus referred to a table which indicated their composition. They were also arranged to be seen with either a light or dark background. The use of this plate was of necessity limited to daylight, but the illustration and description are given as occurring at night in order to show its illuminating power.

At the beginning of the process that which issues from the converter does not appear to be a true flame, but only an illumined stream of gas carrying with it innumerable red-hot pellets of iron. This gas has scarcely any illuminating power, extends but a short distance from the mouth of the converter, and is sometimes sheathed with a whitish smoke. Seen through the glasses the flame and sparks have a deep crimson color, the converter is invisible, and at the base of the flame is a crimson band which continues throughout the process.

As the reaction continues, this stream of gas grows brighter and more elongated, and after a few minutes a small pointed whitish flame appears, which suddenly increases in size. At this instant the blast-pressure falls from twenty to eighteen pounds.

When viewed through the glasses the upper part of the converter comes dimly into view, and the flame and pellets of iron appear of a lighter color, while the fragments of slag which begin to be thrown out are of a deep red. This difference in shade between the iron and slag thrown out is probably entirely owing to the lower temperature of the latter, for the reason that while the iron is discharged from the metallic bath the slag is washed up on the sides of the converter, and can be seen clinging around its mouth in a spongy mass until detached and thrown out by the blast. The greater porosity of the slag and its consequent more rapid cooling would also cause a difference of temperature.

In the second period the discharge of slag increases, and the flame is very bright and illuminating, with occasional dark streaks. Through the glasses at the beginning of this period

the flame is of an ashy blue color with streaks and flashes of crimson; the edges being sometimes of a purplish hue. At this point surrounding objects are illuminated, and the converter becomes distinctly visible. A wreath of crimson is seen surrounding the flame where it strikes the chimney. By the middle of this period the crimson almost entirely disappears from the body of the flame, leaving only a slight cone at its base, and a border of greenish hue makes its appearance, and gradually grows more decided. Streaks of a dark blue color are also seen in the body of the flame.

The beginning of the third period is scarcely indicated to the naked eye, though the flame becomes somewhat weakened, and after a few minutes shows dark streaks running through it. Through the glasses at the commencement of this period the rose-colored cone begins to expand and deepen, the greenish sheath is more decided, while streaks of dark and green are visible. After a few minutes the change becomes very rapid, a few seconds only being required to reduce the flame from rose-color to the deep crimson non-illuminating gas, as at first, and again the converter is lost to view, by which time the blast should have been turned off.

The gradual fading of the crimson from the beginning of the blow and its deepening at the termination of the process, as well as the crimson band at the base of the flame and the wreath of crimson surrounding the flame at the chimney, tend to confirm Mr. Rowan's views, which are, that the different shades of crimson are due to changes of temperature. The stream of gas which comes from the mouth of the converter at the beginning of the process being illumined from within, derives its color from the metallic bath, the temperature of which, owing to the combustion of silicon, increases more rapidly during this period than at any other.

The crimson band at the base of the flame and the wreath of crimson at the chimney might also be accounted for by this theory. The flame rushing from the mouth of the converter has a tendency to create a vacuum at its base around the converter's edge, and thus to cause a wreath of flame to pass over this surface and by consequent cooling produce the crimson band. The wreath of crimson at the chimney may be also due to the cooling of the flame consequent upon deflection.

It is true we have a seeming contradiction to this theory in the rose-colored cone extending from the base at the center, which we would naturally consider the hottest part of the flame; but, as in the flame of the Bunsen burner, the hottest part is in its outer sheath, the conditions of combustion in both being similar, it is probable that that part of the flame occupied by the cone is at a lower temperature than that surrounding it.

The green streaks in the flame are most intense when the manganese spectrum is brightest; and as the color of the flame when the spiegeleisen is added is also green, we are led to suppose them due to the presence of manganese.

On two occasions simultaneous observations were made with the spectroscope and the colored glasses; but with the exception of that just mentioned, and the changes at the commencement and termination of the blow, no striking coincidence was noticed.

II. *Examination with the Spectroscope.*

The science of spectrum analysis is yet in its infancy, and there has been no scientific investigation, perhaps, which has been more contradictory in its results than that of the Bessemer flame. The first application of the spectroscope to the analysis of the Bessemer flame was made in 1862 by Dr. Roscoe at the works of Messrs. John Brown & Co., in Sheffield. Soon after this it was in constant use in Brown's works for controlling the process. It was next introduced at Crewe, and from there said to have been taken to Seraing, in Belgium, in 1865.

Roscoe's account of the general appearance of the spectrum has not altogether been verified by subsequent observers. His not having seen any line beyond 80° indicates an imperfection in his instrument. He, also, is the only one who claims to have seen the sodium line as an absorption band, or who professes to have detected the lines of nitrogen and hydrogen in the Bessemer spectrum. His spectroscope was so arranged that the spectrum of the Bessemer flame was seen in the upper half of the field of view, while the spectrum with which it was to be compared was seen immediately below. The spectrum of the flame was thus compared with the following spectra:—

1. Spectrum of electric discharge in carbonic oxyd vacuum.
2. " " strong spark between silver poles in air.
3. " " " " " iron " " "
4. " " " " " " " " hydrogen.
5. Solar spectrum.
6. Carbon spectrum—oxyhydrogen blowpipe supplied with olefiant gas and oxygen.

The coincidences observed were very few, and totally failed to explain the value of the Bessemer spectrum. The lines of the well-known carbon spectrum did not occur at all, either as bright lines or absorption bands, nor was any coincidence observed between the lines of the Bessemer spectrum and those of the carbonic oxyd vacuum tube. The lines of lithium, sodium and potassium were strongly marked and identified with certainty. He found that three fine, bright lines between E and *b*, shown on the plate at 66½°, 67°, 67½°, coincided with those of iron; and in place of the red hydrogen line C, he dis-

covered a black band which he considered an absorption-band, and states that it is better defined in wet than in dry weather.

In Austria, Prof. Lielegg followed up this subject with great perseverance, and gave more extended accounts of the varying character of the Bessemer spectrum during the different stages of the process. His experiments were made at Gratz, where the spectroscope was afterward used with great success in controlling the Bessemer process; but at Königshütte, where dark gray manganiferous iron was used, it was found that the indications which in other works so plainly determined the moment of decarbonization were unreliable. In this case, the lines whose disappearance is to indicate the exact point of time for ending the process, disappear too soon. During the period in which the spectrum is brightest, among the glowing vapors and gases that stream from the converter, carbonic oxyd next to nitrogen is most abundant, and it is for this reason that the first investigator, Roscoe, expressed himself as confident that the numerous lines of the spectrum were caused by this gas, although he could obtain no coincidence.

Brunner* states that "no part of the Bessemer spectrum is ever visible in the flame when the converter is heated for the first time after being re-lined, but that when the lining is not new, Lielegg's group of green lines ($CO\gamma$) appears in the spectrum, which then contains also the lines of potassium, sodium, and lithium." From which he concludes that this spectrum is not to be identified with carbonic oxyd, but must be produced by other constituents of pig-iron. Others state that the Bessemer spectrum is sometimes visible while the converter is being heated after a blow. I made an observation of the flame from the converter while it was being heated the first time after being re-lined, and obtained with great distinctness the potassium, lithium, and sodium lines, but have not under any circumstances detected any other lines while the converter was being reheated.

Lichtenfels, by a series of simultaneous comparisons of the manganese with the Bessemer spectrum found the lines in the blue and green fields to completely harmonize in the two spectra. The violet manganese line which had been seen by some he could not detect in either of the spectra. I have never observed it, but Dr. Wedding, who has summed up the observations of others, states that he has repeatedly seen it. Its position is at $135\frac{1}{2}°$.

The instrument used in my investigations was constructed by Alvan Clark of Cambridge, and consists of an equiangular flint-glass prism, in a metallic box, into the sides of which at the requisite angles are screwed an inverting telescope with a

* Van Nostrand's Eclectic Eng. Mag., vol. i, page 508.

magnifying power of six, and a tube containing the adjustable slit and lens for rendering the rays parallel; also a tube with a scale, which is placed at such an angle that it is reflected from the surface of the prism through the telescope to the eye; it can be so adjusted as to appear along the upper edge of the spectrum. I was provided with Bunsen's plates of spectra on a large scale, and in order to adapt them to the scale in my instrument, I took the spectrum of the sun and obtained Fraunhofer's lines with great distinctness. Two characteristic lines in the solar spectrum were then noted, one of which appeared at 37° and the other at 117°, and a space measured equal to their distance apart as given on Bunsen's scale. This was divided into eighty equal parts, and the division extended in both directions. By the application of this scale to Bunsen's, I found that the remainder of Fraunhofer's lines in my instrument exactly coincided with their position on his plates. The correctness of the new scale was also proved by other coincidences. By moving the prism, Fraunhofer's lines will vary slightly in their relative distances apart, but in no possible position in which I could place the prism could I obtain the sun-spectrum as given by Wedding in connection with the Bessemer spectrum; if the spectrum given by him was obtained by the use of bisulphid of carbon in his prism, that substance causes a greater variation than I had supposed.

I have recorded the results of twenty-five observations on the Bessemer flame, most of which were taken at a distance of about thirty feet from the flame, though I have stationed myself at intermediate points between that and the flame; at one time sitting so close as to be almost scorched. Nearly all my observations were made at night and the lines obtained much better defined than when seen in diffused sunlight.

The record of my observations was kept as follows:—Five columns were ruled, headed—

| Degree. | Color. | Brightness. | Time. | Remarks. |

Note was made of the dark bands as well as the bright ones, both of which were classed according to their distinctness, as very bright, bright, faint, and very faint. In the time-column was noted the number of minutes after the commencement of the blow at which the lines appear.

At the first two or three observations I attempted to make a thorough note of the changes as they occurred throughout the whole spectrum, but afterward abandoned it as utterly impossible, as at the beginning of the second period, the lines come in so fast and the changes are so rapid that they can not be accurately noted at the exact moment of their occurrence. I therefore confined myself to a few degrees at each observation, and by this method was enabled to note accurately, and at the exact moment of their occurrence slight changes which other-

wise might have escaped notice. Note was also taken of the changes in the general appearance of the whole spectrum during the successive stages of the process. After having made half a dozen observations, while viewing the spectrum of the flame from the converter while it was being heated for another charge, it was discovered that a movement of the eye before the eye-glass occasioned a similar movement of the lines of the spectrum along the scale, on which their position could thus be made to differ more than half a degree. I have seen no notice of this in the statements of others, and it may account for some of the apparent discrepancies. Thereafter, when taking the readings of any of the lines, the position of the eye was so adjusted as to bring the sodium line exactly at 50°. Owing to the extreme brilliancy of the flame the aperture may be made exceedingly narrow, and thus the many lines of the spectrum which with a duller light and broader gauge would be blended together, may be separated.

At the beginning of the blow, the spectrum is continuous and very faint, and generally extends from 35° to 120°, covering about three-fourths of the length attained in the second period. This increases slightly in extent and brightness until the appearance of the sodium line. This line appears at the end of the first period at the beginning of a more decided flame. It comes flashing through from one extremity to the other for an instant, and then disappears only to return the next instant in brighter flashes which are continued for about a minute, by which time the line becomes permanently established. On one occasion the sodium line, instead of flashing and disappearing as usual, continued visible after a few seconds, and expanded and contracted in width almost isochronously until it became permanently established. The appearance of this line indicates the termination of the first period. This period I have found to vary in extent from three to seventeen minutes in blows lasting from thirteen to twenty-seven minutes. None of the other lines make their appearance in vivid flashes as does the sodium. The lithium line becomes visible three or four minutes after the first flash of the sodium. It is very faint at first but soon becomes quite distinct and lasts through the blow. The vivid flashing of the sodium line may be accounted for by the exceedingly small amount of sodium required to produce its spectrum—an amount not exceeding $\frac{1}{1000000}$ of a grain. The slightest momentary combustion taking place in the stream of gas from the converter, would at that instant render glowing a sufficient amount of the vaporized sodium to produce its spectrum, and thus occasion the flashes so characteristic in the first appearance of that line. Lithium exists in a much smaller quantity and requires $\frac{1}{0000000}$

of a grain, or thirty times that given for sodium. By the time the lithium line is established the red potassium line at 23½° and occasionally the violet line at 135° appear, and the blue and green fields become divided into bands which are so rapidly resolved into bright and dark lines, that it is difficult to note the exact time of the appearance of each. The spectrum increases to a dazzling brightness, and extends itself in both directions until it reaches from 23½° to 140°.

During the third period the spectrum becomes more brilliant, and the lines more distinct. Several new lines make their appearance in different parts of the spectrum, of which the ones at 51½°, 57°, and 67° are well defined, while others are faint and not always visible; some of them appearing only toward the close of the last period. In viewing the lines in the most refracted part of the spectrum, it has been repeatedly observed both by myself and others, that these lines were more strongly marked when entering the eye at an angle than when viewed directly. That this was not imagination is proved by repeated identification of lines at the same point on the scale.

At the termination of the blow the lines are rapidly swept away, sometimes in the inverse order of their appearance, but more generally they disappear within the space of two or three seconds, leaving a continuous spectrum as at first, though somewhat brighter. Sometimes the sodium and lithium lines are swept away with the others, and at other times they remain visible. In either case the change is very decided, and does not generally occupy more than three seconds. In the course of my observations, thirty-three lines have been detected, as given in the table below.

Some of the lines given by Lielegg I have failed to find, but have detected others not given by him.

1st Period, 23½, 35, 50, 135.
2d Period, 23½, 35, 43, 44, 44½, 45½, 46, 47½, 48½, 50, 52, 53, 56, 56½, 61½, 62, 62½, 63, 65, 66½, 67½, 70, 72, 120, 135.
3d Period, 23½, 35, 43, 44, 44½, 45½, 46, 47½, 48½, 50, 51½, 52, 53, 56, 56½, 57, 61½, 62, 62½, 63, 65, 66½, 67, 67½, 70, 72, 100, 102, 103, 105, 108, 135.

Among the dark bands detected, the most intense occurred at 44–46, 51–55, 56–58, 62–64½; others were found at 33–34½, 36½, 37½, 38½, 40, 68–72.

Many of the dark bands were crossed by bright lines.

I have repeatedly observed the dark band considered by Roscoe to be a hydrogen absorption line, but have not noticed that its intensity varied with the dampness of the weather. Whether it is an absorption band or not can be determined by a series of observations continued through wet and dry weather. If this proves to be a hydrogen line, the Bessemer spectrum will be found more complicated than is generally supposed. It has

been thought by some that the dark bands in the spectrum are absorption lines due to the cooling of the outer sheath of flame, but it is more probable, that although the pellets of iron and slag tend to produce a faint continuous spectrum; yet in contrast with the very brilliant lines it appears discontinuous, the dark bands being merely intervals between the bright ones. The iron spectrum has not been satisfactorily identified. It has been suggested that the brightness and size of the lines of the Bessemer spectrum do not allow the iron lines to appear. In comparing the Bessemer spectrum with Bunsen's spectra of nickel, cobalt and calcium, no coincidences were observed except two or three in the latter spectrum. The brightest calcium line, however, was not visible in the Bessemer spectrum. The Bessemer spectrum contains yet many mysteries to be solved, among which is the cause of the non-appearance of the lines of the spectrum at the beginning and termination of the blow.

This was readily solved when the numerous lines of the spectrum were attributed to carbon, but in proving them to be caused principally by manganese, their disappearance is not so readily accounted for.

One theory to account for it is that the luminous power of the flame is too small at the beginning and end of the process to produce a spectrum. In regard to this it may readily be shown that the brilliancy of the spectra of incandescent metallic vapors does not depend upon the illuminating power of a flame but upon the heat of the flame into which they are introduced. For instance, the spectra are more distinct in the non-luminous flame of a Bunsen lamp than in the ordinary luminous gas-flame. If we take the theory as referring to the feebleness of light given off by those substances in the flame which produce the spectrum, it will resolve itself into the one of change of temperature, notwithstanding the fact that the illuminating power of flames of the same temperature varies with the composition of the gas, because there is evidently enough sodium in the flame to give its characteristic line; hence, whatever might be the illuminating power of the flame, if the heat is sufficiently intense the sodium line *will* show itself.

Dr. Wedding adopts the theory that the absence of the spectrum at the beginning and termination of the blow is because the absolute quantity of the bodies volatilized producing the spectrum is at these times too small. His reasons for holding this view are as follows:—"A trace of sodium will give its characteristic line, but, according to Simmler, a much larger quantity of manganese is needed to obtain a recognizable reaction than that which can be detected by the well known blow-pipe reaction with carbonate of soda. Consequently, spectrum analysis does not depend alone upon the *presence* of a body but also upon the

presence of a certain quantity. And although manganese is always left in the iron, it may not be left in sufficient quantity at the termination of the blow to produce the spectrum, and for this reason the lines disappear."

To this theory there are same some strong objections. 1*st.* If we take manganese in sufficient quantity and hold it in a flame the spectrum will increase in brightness until a uniform temperature is attained; but when the amount of manganese vaporized begins to diminish, its spectrum will gradually decrease in brightness until it disappears. Now, if the disappearance of the manganese lines in the Bessemer spectrum is owing to the diminution of the quantity of manganese, we should infer that these lines would gradually grow more indistinct and then fade away; but on the contrary, the manganese spectrum increases in brilliancy from its first appearance, and is more intense just before being swept away than at any other time. The analysis of the smoke, which appears when the flame ceases, proves that a considerable quantity is still volatilized, and it is notable that in manganiferous iron this quantity increases towards the close of the blow. 2*nd.* It would be more difficult to account by this theory for the non-appearance of the sodium line at the beginning of the blow, as sodium then in all probability exists in the issuing gas in sufficient quantity to produce its spectrum at a high temperature, as it is only by special precaution that we can keep it out from any flame. 3*rd.* A still greater difficulty would arise in applying this theory to the spectra of sodium and lithium at the close of the blow. As has before been stated, these lines sometimes disappear at the moment of complete decarbonization, and sometimes remain. In the former case, to say that our friend sodium had given out would be doing great injustice to that element, as it has never given us reason for bringing so grave a charge against it. Dr. Wedding in attempting to demonstrate that the non-appearance of the manganese lines is owing to the lack of sufficient quantity volatilized to produce its spectrum, makes the following statements:—

From analyses made by Brunner we find that the manganese contained in the iron falls from 3·460 per cent in the raw material, to 1·645, 0·429, and finally to 0·113 per cent in the decarbonized product; and that the protoxyd of manganese in the slag first increases from 37·00 per cent to 37·90 per cent, and then sinks to 32·23 per cent, and furthermore, that a certain quantity of manganese is to be found in the smoke. How much manganese is really lost by volatilization cannot be determined, since data are wanting as to the absolute quantity of slag and iron, consequently we cannot determine how much manganese has been lost by means of the eruptions.

But since the manganese contained in the pig-iron decreases

constantly, and that contained in the slag after the termination of the boiling period also decreases, a considerable volatilization of this body is probable just at the time when the spectrum is best developed. Comparing with this the experiments that can be made in the laboratory we arrive at the hypothesis, that the oxydized manganese which has entered into the slag is not volatilized but is retained by the slag; it can, therefore, get into the flame only in the shape of solid or fluid combinations.

In the above statements the results of the analysis prove that some of the manganese in the slag is volatilized. We cannot consider the manganese spectrum during the entire process as due wholly to the volatilization of the manganese directly from the iron, for while the amount eliminated from the iron grows continually less, the manganese spectrum grows brighter. Owing to the intimate mixture by the blast of the iron and slag, the manganese oxyd contained in the latter, is brought in contact with the melted iron and vaporized. This mixing of the slag and iron would cease at the termination of the process, and this would account for the sudden diminution of smoke.

If there was a sufficient carbonic oxyd flame to render the escaping gases glowing it is evident they would not issue from the converter as dark smoke, but as incandescent vapor having its characteristic spectrum. The lack of sufficient flame may, therefore, account for the disappearance of the manganese spectrum. The Bessemer flame presents other problems, and opens an intensely interesting field for scientific investigation; and by the use of more delicate instruments than have yet been employed for this purpose, discoveries may be made which will throw new light upon the subject of spectrum analysis.

ART. XXIX.—*On a simple method of measuring Electrical Conductivities by means of two equal and opposed magneto-electric currents or waves;* by ALFRED M. MAYER, Ph.D.

[Read before the Troy meeting of the American Association for the Advancement of Science.]

1. *General description of the Method.*

A MAGNET is firmly supported in a horizontal position with a portion of its length projecting beyond a fixed stop (see fig. 2); over this free end of the magnet, and resting against the stop, are placed two similar flat spirals, formed of the same quality of copper wire, and having the turns of one spiral in a direction the reverse of those of the other. The spirals are clamped together and their four terminal wires are carried vertically downward into four separate cavities containing mercury; these mercury-cups are so connected with a reflecting-galvanometer

that, when the spirals are together slid off the magnet, the two equal electric currents, thus generated, simultaneously tend to traverse the galvanometer in opposite directions, and therefore its needle remains stationary. If we now introduce into the circuit of one of the spirals a resistance equal to that introduced into the circuit of the other, the needle will still remain at rest when the spirals are slipped off the magnet; but, if the resistance placed in one circuit is greater or less than that placed in the other, there will be a deflection of the galvanometer needles when the spirals are removed. Thus, by introducing wires of different metals into the circuits we can readily determine their relative conductivities, by making them of such length that their resistances are equal; which condition is attained when, on sliding off the spirals, the needle remains absolutely at rest. If, in the latter case, the wires have equal diameters then their conductivities are directly and their resistances are inversely as their lengths.

A modification of the above method is discussed in the conclusion of this paper; in which the magnet is replaced by the terrestrial magnetic force and the spirals and the wires by two similar coils, from two to three feet in diameter, formed of the two wires whose conductivities are to be compared. These coils contain equal lengths of the same sized wires and the same number of turns; the direction of the turns being opposed in the two coils. The coils having been bound together are placed in a plane at right angles to the line of "the dip," and the four terminal wires are so connected with the reflecting-galvanometer that the two induced currents tend to traverse it in opposite directions. The coils are now quickly rotated through 180°, around an axis at right angles to the line of the dip, and if the wires present equal resistances the needle remains at rest; if it is deflected, the direction and the amount of the deflection shows which coil has the lesser resistance and affords a means of estimating the same.

After this general description of the method I will present, in order, a description of the apparatus used, and of the actions which take place in it; the degree of precision of the method; examples of the determinations of electrical-conductivities, and experiments on the modification of the method.

2. *Description of the Apparatus.*

The magnet was formed of a combination of three steel bars, separated from each other by slips of wood ·2 in. thick. The middle bar was 10·4 in. long and its ends projected ·25 in. beyond the two side magnets. Each bar was ·27 in. thick and ·9 in. wide. About three months before this investigation was undertaken they had been magnetized to saturation by the

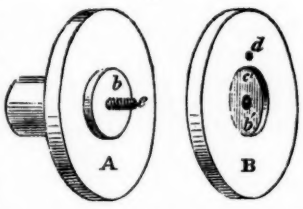
following process. The axis of a helix, 8·7 ins. long and containing 558 feet of $\frac{1}{16}$ in. copper wire, was placed in the line of "the dip" and a current so sent through it from ten Bunsen cells that its N. pole was toward the earth. The separate bars were then drawn through the helix until they ceased to acquire an increase of magnetism. This method gives a uniform and powerful magnetization, and probably may be improved by causing the bars to vibrate as they pass through the helix; which can be accomplished by means of a tuning fork furnished with a long brass stem. After the magnets were combined, as described above, a weight of 1·5 lb. was sustained at the end of the middle bar.

The magnet was supported in an E. and W. line, 15·5 in. above the surface of the mercury in the cavities of the wooden "connecting-block" placed below it; and 2·5 in. of its S. end projected beyond the wooden clamp which held it.

The Spirals were formed of $\frac{1}{32}$ inch "double-covered" Lake Superior wire. Each spiral contained 176·06 in. of wire coiled in 20 turns, and the terminals were 15·5 in. long, thus making 207·06 in. of wire in each spiral. The greatest diameter of the spirals was 3·9 in. and each had a central opening of 1·7 in. Their thickness after they were covered with paraffined paper and varnished was ·06 in. The covering of the two terminal wires of each spiral was saturated with melted paraffin; they were then firmly tied together with silken cord to about ·4 in. of their ends where they separated and formed forked terminations.

The spirals were formed in this manner. An iron plate A, which screws on to the mandrel of a lathe, has cemented on to its face a disc of hard wood *b*, 1·7 in. in diameter and ·1 in. thick. From the center of the plate A projects a screw *e* which enters the wooden disc B at *e'*. When the plate B is screwed "home" the disc *b* fits into the cavity *b'* and the plates A and B are separated to a distance a little greater than the diameter of the covered wire, while the disc *b* forms a cylinder between them on which to wrap the spiral.

1.



The end of the wire to be coiled is passed through a hole *d* in the plate B, which is then screwed home on to A. The lathe is then turned so that the wire is coiled over the center disc from A to *b*. After the space between the disc is filled with coils, the free end of the wire is secured and the plate B unscrewed, while the wire slides through *d* and the coil is not unwrap-

ped; which would have taken place if it had been coiled in the direction from *b* to A. The spiral is now saturated with very fluid paraffin and has cemented on to it, with a hot chisel, a paper disc previously saturated with paraffin. The spiral is now removed, the covered side placed against the disc and its other surface treated in the same manner. The spiral is then taken off the chuck, and on holding it up to the light the copper wire is distinctly seen through the translucent covering of the wire and the paraffined paper cover of the spiral. The insulation thus obtained is very perfect and the coils are firmly cemented together. The terminals are now led radially from the spirals, and are tightly bound together as described above. To still further strengthen the spirals, both they and their terminals are covered with a firm layer of shellac varnish.

I have thus minutely described the process of making these spirals for they are of inestimable value in many electrical researches; having been used in my recent investigations in electro-magnetism, and will be again used and referred to in a subsequent communication.

The galvanometer I specially constructed for this research, but experience has shown that a coil of shorter and thicker wire, (say, $\frac{1}{16}$ in. wire in 6 turns) offering less resistance, would have been better than the one employed. The wire of the galvanometer was $\frac{1}{26}$ in. thick, and was wrapped around the lower needle in two layers of 22 turns each; the opening of the coil being ·15 in. in width. The needles are 1·65 in. long and ·03 in. diam. The upper needle is ·6 in. above the lower with a thick copper plate intervening; it was not much affected by the current in the coil, and was under the influence of a feeble magnet, 13 in. long and ·18 in. diam., which was placed with its similar poles over the upper needle and 8·2 in. above it. Under these conditions the simple oscillations of the system were exactly $4\frac{1}{2}$ per minute, and by lowering or raising the magnet I could render it more or less astatic. The needles were hung by a frame of fine copper wire to a plane mirror 1·2 in. square, formed of thin glass silvered by Foucault's process, and the whole was suspended by a few fibers of unspun silk. The instrument was enclosed in a cover, the front of which was made of a carefully selected piece of plate glass.

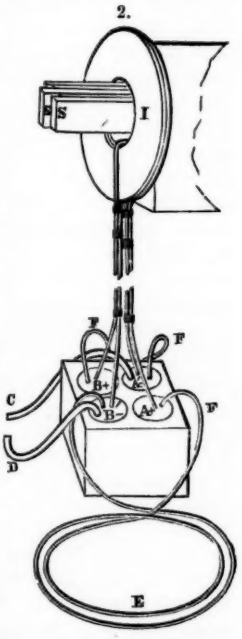
The S. end of the magnet used in inducing the electric currents in the spirals was 14 in. to the left of the magnetic meridian line drawn through the point of suspension of the galvanometer needles and 6 ft. 11 in. distant from the same. In this position the magnet caused a deflection of 52′·5 in the needles of the galvanometer and they were brought back into the meridian by means of the damping-magnet.

The deflections of the needles were read off by the beautiful

method invented by the illustrious Gauss, using a telescope and scale placed as described below.

The telescope was of ·7 in. aperture and 12 in. focus; just under its object glass was placed, at right angles to its axis, a rod of wood 1 in. square and 1 meter long, covered with drawing paper. This rod was divided off into centimeters by lines 1^{mm} thick; thus, the division lines were $\frac{1}{10}$ of the distance between two similar sides of the centimeter divisions. A thick spider thread was selected which just covered a division line, and therefore was also, apparently, 1^{mm} thick. By this simple device,—using one and the same side of the spider line as point of reference,—we can accurately estimate deflections of the needles corresponding to $\frac{1}{10}$ of a division of the scale.

The scale was 2·285 meters distant from the center of the mirror, and therefore a motion of 1 division of the scale over the spider thread corresponded to an angular deflection of 7′ 30″, and as we have seen that $\frac{1}{10}$ of a division can be accurately read, it follows that we can determine a deflection of 45″. In this paper I will give the deflections in divisions of the scale, which can be converted into minutes of arc by multiplying them by 7·5.



Connecting-block is the name I give to the block of wood, placed under the projecting end of the magnet; it has four cavities containing mercury, by means of which we make the various electrical connections required in the experiments. Fig. 2 gives a view of this block and shows the manner of making the connections when the object is the measurement of relative electrical resistances. Four holes, 1 in. in diam. and 1 in. deep, separated by walls ·1 in. thick are bored out of a block of wood, and then coated with thick shellac varnish. A, A′ are the terminal wires of one spiral, B, B′ those of the other. The wires to be compared are at E and F. If E represent the standard wire of a fixed length, then the wire F has to allow of its length being altered so that its resistance may be made equal to that of E. This is arranged by sliding one end of this wire through a heavy copper clamp (not shown in the fig.) which is fixed in the mercury-cup B, while the other end, previously well amalgamated, dips into cup A′.

The two spirals I are firmly clamped together, so that when slid quickly off the magnet S, they both have the same direction of motion. By referring to fig. 2, the directions in which flow the currents, thus produced, can be readily followed. Taking spiral A A′, the current flows down the terminal A+ through the wire E into B′, thence by the wire D through the galvanometer and back by the wire C to A′−, the other leg of the spiral, thus completing the circuit. In the case of the spiral B B′, the current flows down the terminal B+ through the wire F, thence by the wire C through the galvanometer; returning by D to B′, thus forming the circuit.

It is evident that if the spirals by themselves generate equal currents, and the resistances E and F are equal, no deflection of the needles will ensue, for equal and contrary currents will tend simultaneously to traverse the galvanometer.

The manner in which contacts are made in these experiments is of great importance. The double silk covering of the wires is unwrapped to ·2 in. from their ends; the unwrapped silk is then firmly wound over the end of the silk covering and saturated with thick shellac varnish. The uncovered end of the wire is now scraped, rubbed with nitrate of mercury, and well amalgamated, up to the silk covering. The iron wires were amalgamated by dipping their uncovered ends into sodium-amalgam. Thus, even if the end of the wire should dip deeper than ·2 in. into the mercury, the point of contact will yet remain at that distance from the end, as the shellac prevents contact above the amalgamated portion of the wire. The terminals of the spirals and of the galvanometer coil were formed in the same manner. One end of the wire whose resistance was to be compared to the standard copper wire, was uncovered and well cleaned for some portion of its length, so that it could be drawn through the heavy copper clamp until its length equalled in resistance the standard wire. The wires were then removed and their lengths accurately measured.

3. *Investigation into the actions which take place in the apparatus.*

In the general introductory description given of the method, I have, for simplicity of illustration, assumed that when the two spirals,—similar as to form, length of wire and resistance,—are slid off the magnet, no current would be sent through the galvanometer. But this cannot be, for the hinder spiral is further on the magnet than the other by ·06 in. and therefore cuts more "lines of magnetic force," and also, the two spirals traverse simultaneously portions of the field differing in magnetic intensity.

The following experiments will exhibit the above action. I will call the back and front spirals respectively A and B.

They were reversed on each other, clamped together and had their terminals dipping in A and A′ so that the two induced currents would tend to traverse the galvanometer coil in opposite directions.

The mean of three experiments shows that when the spirals are slid off there is a deflection of 2·43 div. in favor of spiral A. But, on making spiral B the back spiral, the needle moved 3·9 div. in its favor; thus showing that spiral B offers less resistance than A, though both lengths were contiguous pieces taken from the same sample of wire. On again placing A against the stop, I found that a resistance of 2·9 in. of $\frac{1}{2}\frac{1}{0}$ in. wire, when attached to one leg of this spiral, reduced its action to equal the forward spiral and the needle remained absolutely unaffected when the spirals were quickly removed from the magnet.

The following experiments show the effects of separating the spirals. The balanced spirals remaining, in other respects, as in the last experiment, I separated them ·05 in. by intervening card-board; the needle was now deflected 1·05 div. in favor of the back spiral, and on increasing the separation to ·125 in. the action of the back spiral equalled 3·6 div. of the scale.

I have said above that the two opposed currents *tend* to traverse the galvanometer coil, because theoretic considerations induce me to hold the opinion that two currents cannot simultaneously traverse a wire in opposite directions, and that only the excess of the intensity of one current over the other is really propagated through the wire.

The next point to be considered is the mutual inductive action of the spirals. The directions of the turns in the spirals being opposed, and as the current in each, on sliding them off the magnet, rises rapidly to a maximum intensity and as quickly comes to 0, it follows that they must exert a mutual inductive action. But although the current in one spiral during the rise to its maximum causes an induced current in the other spiral in the same direction as that induced in it by the magnet, yet, as the current *decreases* as quickly to 0 after it has reached its maximum, it follows that a current in the opposite direction to that induced by the magnet in the other spiral will now quickly follow it, and as these currents, +, and − are equal, there will be no increased outside effect arising from their interaction; and many experiments showed that whether a copper disc was placed between the spirals or an equally thick disc of paper, the action at the galvanometer was the same.

The following experiments on this subject appear to confirm the above view. The two spirals were placed on the magnet, but only the front one was connected with the galvanometer, while the terminals of the back spiral were separated so that no

current went through it when the spirals were together slid off the magnet. The action of one spiral alone was sufficient to deflect the galvanometer needle about 60°. This deflection was reduced to 1° 18′ by placing in its circuit a helix of 735 ft. of no. 18 copper wire; the mean of six experiments (the *range* of which was only $\frac{1}{10}$ div.) giving 10·4 div. The ends of the back spiral were now so connected that an equal current flowed through it in a direction the reverse of the other. The mean of six deflections, produced by sliding together the spirals off the magnet, equalled 10·4 div., the same as in the previous experiment; thus showing that the mutual inductive action of the spirals had no effect on the intensity of the induced magneto-electric currents.

It was also found that on passing the induced current from a spiral through another spiral on which rested a third spiral whose ends were connected with the galvanometer, that no deflection ensued when the magneto-electric current was passed through the inducing spiral.

However, the magneto-electric currents were of such low intensity that probably they were not able to produce an induced current in the second spiral capable of deflecting the needle, and that therefore the experiments here narrated are of little value; nevertheless, I think the reasoning given above will be supported by experiments made with more powerful magnets and with larger spirals.

4. *The degree of Precision of the method.*

The degree of precision of this special apparatus was determined in the following manner. A copper wire 123 ins. long had opposed to it a resistance which was about equal to 120 ins. of its length and the mean deflection of the galvanometer-needles was carefully determined. The copper wire was now shortened 1 in. and the deflection again determined; this was repeated,—determining the amount of deflection produced after each shortening of 1 in.,—until 6 in. had been cut off. These experiments showed that a diminution or increase of resistance of $\frac{1}{120}$ part in one of the wires caused a deflection of ·4 div. of the scale, or of 3′ of arc, in the galvanometer-needles. But we have seen that ·1 div. can be read on the scale, therefore, we can, with this special apparatus, detect and measure an increased or diminished resistance of $\frac{1}{480}$ part. But as the galvanometer can be removed to even twice the distance at which we read its deflections, I think I am safe in saying that with this method, as applied with the above apparatus, we can measure a difference of resistance in two conductors of $\frac{1}{300}$ part; which is far within the variations observed in different samples of wires of the same lengths and diameters.

If a galvanometer formed of 6 or 8 turns of ·1 in. wire were used in connection with a powerful magnetic battery and larger spirals of thicker wire, while the galvanometer is placed at a greater distance, I have no doubt that a variation of $\frac{1}{1000}$ part can thus be detected and measured.

5. *Examples of the determinations of electrical conductivities by this method.*

The object of these determinations was not to furnish science with new and accurate data,—for that would have required a careful personal supervision of the operations of preparing chemically pure metals,—but it was to give examples setting forth the practice of the method.

I had prepared "hard-drawn" wires, of No. 18 B. W. G. (= ·049 in. diam.), of copper, silver, iron, and German silver. These wires were found to have the same diameter. They were all covered with a double wrapping of silk.

Silver.—The spirals were balanced, by the introduction of an increased resistance in the back-spiral, so that no deflection took place on sliding off the spirals. A length of 120 in. of the silver wire having been placed in the circuit of one spiral, it was found that 127 in. of copper wire were required in the other circuit, in order to equal it in resistance. Taking the copper wire as the standard of comparison, at 100, we have

$$127 : 120 :: 100 :: 94{\cdot}48.$$

Matthiessen (Phil. Trans., 1858, 1862) makes the ratio of the conductivity of silver to copper, both hard-drawn, as 100 : 99·95 or about equality; but in my determination the silver is 5·5 per cent below the copper. I therefore suspected impurities in the silver, and an examination of the wire kindly made by my colleague, Dr. Wetherill, showed that it contained about ·01 per cent of gold and a trace of iron. This accounts for the low number found, and affords a good illustration of Pouillet's remark, that the purity of a metal is most readily determined by a measure of its electrical conductivity. The electrical test of purity, however, exceeds in delicacy the chemical examination; for a very minute percentage of alloy causes a great increase of resistance, and if we could be sure that the wires we compared were in the same physical condition as to annealing or hardness, we could probably use this method as a means of determining the percentage of a *known* metal which formed the alloy.

Pouillet shows (Traité de Physique, 1856, vol. i, p 606) that silver whose conductivity is 100 when pure, is only 51 when it contains ·037 of alloy, and is 47, 42 and 39 when it contains respectively ·100, ·143, and ·253 of alloy. Pure gold gave 39, but ·049 of alloy reduced its conductivity to 13; and Jenkin

has found that an alloy of 1 part of silver and 2 of gold presents almost as much resistance as German silver.

Iron.—The three following determinations were made of the conductivity of the best quality of iron wire relatively to the standard copper wire.

(1)	The resistance of	240	in.	of	copper wire	=	36·7	in.	of	iron wire.
(2)	"	"	"	111·6	"	"	"	"	=	16·16	"	"	"	"
(3)	"	"	"	60	"	"	"	"	=	8·67	"	"	"	"

Giving for the relative conductivity of iron,

(1)	240	:	36·7	=	100	: 15·29
(2)	111·6	:	16·16	=	100	: 14·48
(3)	60	:	8·67	=	100	: 14·45
						14·74 = Mean.

E. Becquerel (Ann. de Ch. et Phys., III, xvii, 266) gives 13·6 for the conductivity of iron, copper being 100; and both wires hard-drawn; while Matthiessen (Phil. Trans. 1858, 1862) determines 16·81 as the conductivity of iron, copper being 100, and both hard-drawn.

The mean of Becquerel and Matthiessen	= 15·20
My determination	= 14·74
Difference	= ·46

The copper and iron wires in (3) were cut off from the lengths used in (2); but the wires used in (1) were taken from parts of the coils removed from the lengths (2) and (3). This accounts for the close agreement of (2) and (3) and the higher number obtained in (1).

My determination therefore appears to compare favorably with those made with different methods by these experimentalists. I say "appears," because although the copper was of excellent quality and the iron the best procurable, yet they were not chemically examined as to their purity.

Another series of determinations was obtained by comparing the lengths of copper and of iron wires which would equal in resistance one and the same length of German silver wire, used as a term of comparison. The result agreed with the above determinations.

6. *On a modification of the method.*

As long ago as 1832 Faraday (Exp. Res. 170–180) first obtained an electric current, directly induced by the earth's magnetism, by rotating a closed wire circuit around an axis at right angles "to the line of the dip;" an experiment whose theoretic beauty has ever been the admiration of natural philosophers.

A length of 38 ft. of $\frac{1}{16}$ in. insulated copper wire was wound into a coil of 3 ft. in diameter, containing 4 turns. The terminals of this coil were connected by binding screws with the

wires leading to the galvanometer and the coil placed in a plane at right angles to the line of the dipping needle. On quickly rotating the coil through 180° the needles were deflected 25°, and by making the rotations correspond in direction and time with the oscillations of the needle, I found that six rotations brought the deflection to over 45°. Faraday (Exp. Res. 202–213 and 3145 *et seq.*) has shown that the intensities of the magneto-electric currents induced in wires of different metals are as their electrical conductivities, therefore a coil of iron wire similar in all respects to the above copper coil will give a deflection of about 4° for the first rotation; but by increasing the number of turns of the coil to 10 or more and by using a galvanometer with a shorter and thicker wire coil, the angle of deflection can no doubt be doubled.

The above facts show that we can substitute for the steel magnets, previously used, the magnetism of the earth, and can replace the spirals by two similar coils made of the two specimens of wire to be compared. The coils are placed on each other so that their convolutions are in opposite directions; and having been firmly tied together their plane is made to coincide with a direction at right angles to the dipping needle, while their terminals are so connected with the galvanometer that the currents induced in the two coils tend to traverse it in opposite directions.

Things being arranged as above, it is evident,—as the wire coils are similar in all other respects,—that if the conductivities of the wires are the same, there will follow no deviation of the galvanometer needle when the coils are quickly rotated through 180°; but if the wire of one coil offers a greater or less resistance than that of the other the needle will be deflected. By ascertaining what differential actions correspond to known differences of conductivity of coils of a certain diameter, number of turns and thickness of wire, we can, by always using similar coils in these relative measures, ascertain what difference in relative conductivity corresponds to a certain angle of deflection; the chords of these angles, or, the sines of half of the angles, being to each other as the intensities of the currents.

Minute differences of resistance in the two coils may be made to cause a deflection in the galvanometer needle by knowing the time of its oscillation, and by reversing the motion of rotation of the coils so as to correspond to the swing of the needle; thus after several reversals a motion is given to the needle which could not have been observed after a single rotation.

In point of ready application,—and especially in reference to the determination of the resistances of lengthy conductors,—I doubt whether this method will be generally adopted; but after the conception of the idea it appeared worth investigating; this

I have done, and have thus developed at least any value it may possess. It certainly presented an interesting problem and the pleasure afforded in its solution has repaid me for the considerable labor which it required.

S. Bethlehem, Pa., July 15, 1870.

ART. XXX.—*On the supposed absence of the Northern Drift from the Pacific Slope of the Rocky Mountains;* by Dr. ROBERT BROWN, M.A., F.R.G.S., etc., Edinburgh, Scotland.

IN some interesting remarks addressed to the California Academy of Sciences on the 4th of June, 1868, and published in their 'Proceedings' for that year (vol. iii, pp. 271, 272), Professor J. D. Whitney denies that there is any true Northern Drift within the State of California. "Our detrital materials," the learned Professor remarks, "which often form deposits of great extent and thickness, are invariably found to have been dependent for their origin and present position on causes similar to those now in action, and to have been deposited on the flanks and at the bases of the nearest mountain ranges by currents of water rushing down their slopes. While we have abundant evidence of the former existence of extensive glaciers in the Sierra Nevada, there is no reason to suppose that this ice was to any extent an effective agent in the transportation of the superficial detritus now resting on the flanks of the mountains. The glaciers were confined to the most elevated portions of the mountains, and although the moraines which they have left as evidences of their former extension are often large and conspicuous, they are insignificant in comparison with the detrital masses formed by aqueous erosion. *There is nothing anywhere in California which indicates a general glacial epoch during which ice covered the whole country, and moved bodies of detritus over the surface* independently of its present configuration, as is seen through the Northeastern States."

Mr. Whitney goes on to observe that the same condition of things prevails in Nevada and Oregon, the detritus seeming always to be accumulated at the base of the mountains. Further, from the observations of Messrs. Ashburner and Dall, he remarks that "it would appear that no evidences of a Northern Drift have yet been detected on this (Pacific) coast, *even as far north as British Columbia and Russian America* (Alaska). Neither of these gentlemen has observed any indication of a transportation of drift materials from the north toward the south, or any condition of things similar to that which must have existed in the Eastern States during the diluvial epoch." Mr.

W. H. Dall, the gentleman referred to in the foregoing extract, (and well known to the readers of this Journal as one of the most active and observant of the staff of Naturalists attached to the Collins Overland Telegraph Expedition), follows suit to these observations of Professor Whitney by declaring, in a paper published in this Journal for January, 1868, that though he had carefully examined the country over which he had passed, in Alaska, for glacial indications, he had not found any effects attributable to such agencies. His own opinion, indeed, from what he had seen of the west coast, *though yet unproved*, was that the glacier-field never extended in these regions to the westward of the Rocky Mountains, though single glaciers have existed and still exist between spurs of the mountains which approach the coast. No boulders, according to Mr. Dall, such as are common in New England, no scratches or other marks of ice action had been observed by any of his party, though carefully looked for.

It is this general theory of the absence of the Northern Drift in Northwestern America that I propose combating in the remarks which follow, and I do so with extreme diffidence, knowing well from old experience the care and caution with which Prof. Whitney has proceeded in his remarkable geological survey of California, as well as in his earlier work on the shores of Lake Superior. For this reason I will speak only of what I know from personal knowledge of the districts visited by myself, calling in, however, the observations of others as corroboration of my statements.

As far as Alaska and California, and even Oregon and Washington Territory, are concerned, I must leave the question of glacial remains within their boundaries, to observers more intimately acquainted with their country than I am, though I have a strong inclination to believe that what I say about other portions of the Pacific coast will hold equally good regarding them also. I have certainly visited and traveled through California, and have been in some portions both of Oregon and Washington Territory, and on the borders of Alaska, yet my knowledge of these countries does not entitle me to dispute statements so explicitly made by such excellent observers as those cited. But with the coast of British Columbia and the whole of Vancouver Island I am very intimately acquainted—perhaps more intimately than any other single individual—and can speak positively regarding the marked presence of true Northern Drift there, so that with every respect to the opinion of so distinguished a geologist as Prof. Whitney, I am compelled to dissent from his theory regarding the entire absence of glacial remains proper, from the Pacific slope of the Rocky Mountains.

Between 1863 and 1866—nearly four years—I traveled on foot and in canoes, through the forests, over the mountains, on the rivers, the lakes and the prairies of the whole of the region indicated, as Commander and Government Agent of the First Vancouver Exploring Expedition, and as Botanist of the British Columbia Expedition. Again I have three times visited the Arctic Regions, passing a whole summer in Greenland, studying these and other similar phenomena, and have for many years been very familiar with the remains of the Northern Drift in Scotland, the north of England and portions of the north of Europe. These personal particulars are mentioned to show that I am in a position to know glacial remains when found, and to distinguish them from the ordinary terrestrial debris accumulated by causes now in action in the temperate countries where formed. The result of these extended observations has, therefore, been to confirm me in an opinion entirely contrary to that expressed by Messrs. Whitney, Ashburner and Dall, viz: that so far from the Northern Drift being absent from Vancouver Island and British Columbia, it is present in as marked a manner as ever I saw it in countries celebrated for the presence of such remains. This opinion I casually expressed in 1869 in a memoir entitled *Das Innere der Vancouver Insel*, published in the volumes of Petermann's *Geographische Mittheilungen* for that year, and more recently and more explicitly, in another, *On the Physical Characteristics and Geographical Distribution of the Coal Fields of Northwest America* (Transactions of the Geological Society of Edinburgh, 1868–69). As that statement has been inclined to be called in question—scientific sceptics not unreasonably considering that a doctrine promulgated by so eminent a geologist as Prof. Whitney is entitled to further consideration, than a mere curt denial of its truth, I have considered it proper to present in a concise manner in this place the facts on which I base my disbelief in its truth. As early as 1860, Mr. Henry Bauermann, geologist of the British Northwest Boundary Commission, made many observations on this subject; and subsequently, in 1862, in a *Prize Essay on Vancouver Island, its resources and capabilities as a Colony* (Victoria, 1862), Dr. Charles Forbes, R.N., published similar facts, which my own researches have only tended to confirm and enlarge, over a greater area. Dr. Forbes showed, what is familiar to every one visiting that section, that in the whole southern portion of the Island, though from the open prairie-like character of some portions of the southeastern section it is there earlier observed than in the wooded districts, the scooping, grooving, and scratching of the rocks by ice action is very marked. The chief rock *in situ* there is a dense, hard, feldspathic trap, and this is ploughed in many places into furrows six to eight inches deep, and from six

to eighteen inches wide. The ice action is also well shown in the sharp peaks of the erupted, intruded rocks, having been broken off and the surface smoothed and polished, as well as grooved and furrowed, by the ice action on a sinking land, giving to the numerous promontories and outlying islands which here stud the coast, the appearance of rounded bosses between which the soil is found to be composed of sedimentary alluvial deposits, containing the debris of tertiary and recent shelly beaches, which have, after a period of depression, been again elevated to form dry land, and to give the present aspect to the physical geography of Vancouver Island.

The whole surface of the country is strewn with erratic boulders. Great masses of 60 to 100 tons in weight,—chiefly of various igneous and crystalline, as well as sedimentary rocks, sufficiently hard to bear transportation,—are found scattered everywhere over the island from north to south, and through the region lying on the western slope of the Cascade Mountains. Some of these syenitic or granitic boulders are of a fine grain and accordingly some of the chief buildings in Victoria are built from them. I am not aware that any rock of a similar description is found *in situ* anywhere in Vancouver Island; it appears to have drifted in icebergs from the north. I am cordially of opinion with Dr. Forbes, that though the last upheaval of the land, which might have taken place at a geologically recent period, failed to connect Vancouver Island with the mainland of North America; it was at all events sufficient to effect to a great extent, the junction of numerous insular ridges, and thus to form a connected whole of what *was*, and might have continued to be, only an archipelago of scattered islets. The upheaving force elevated and connected these and brought to the surface, the great clay, gravel and sand deposits of the northern Drift which had swept over, and been deposited on, the submerged land. These sands, gravels and clays, were now to form the soil of land, prepared for the habitation of man. These constituents of the drift remain, in many parts, thinly covered by a coating of vegetable mould; but much has been washed away. The clay remains most generally and widely spread out, as a retentive sub-soil, having resting upon it a thick coating of vegetable mould. The most valuable soil is found sweeping down the sides of gentle slopes, filling up hollows and swampy bottoms, and, mixed with the rich alluvial deposits of such districts as Saanich, Cowitchan, Delta of Nanaimo, and Comax, forms an inexhaustible source of agricultural wealth. The true glacial or boulder clay is found in various portions of Vancouver Island. Bauermann has described it as seen near Victoria, and I am glad to be able to vouch for the correctness of his description: it is extensively developed not only there

but on the opposite coasts of Washington Territory and British Columbia. In the neighborhood of Esquimault and Victoria, the rocks are deeply scored and grooved along the shore, and large boulders are scattered irregularly over the surface of the country, as already described. The other rocks observed as erratics were black cherty conglomerate, similar to that underlying the secondaries,* dark laminated mica schist with well defined garnet-crystals, hornblende rock and largely crystalline greenstone, and rarely and in small masses vesicular obsidian and pitchstone.

The following section is given to show the general character of the drift, at Esquimault Harbor.

Black sandy and peaty ground with broken shells	2 to 6 feet
Yellowish sandy clay with casts of shells (Cardium and Mya) and a few pebbles and boulders	6 to 8 "
Gravel of scratched pebbles resting on rock	2 to 3 "

The rocks are grooved and scratched at the junction; the direction of the glacial markings is between N.–S. and N.N.W–S.S.E. In a well sinking at Esquimault Barracks (for the boundary Commission) the lower gravel was reached at 42 feet, after going through a sandy blue clay without shells or boulders. The section in the cliff between Albert Head and Esquimault is as follows:—

Blue drift clay with boulders; junction with rock not seen,	70 feet
Fine sand and gravel, passing upward into coarse quartzose gravel,	100–120 "

—*Quart. Journ. of Lond. Geol. Soc.*, 1860, p. 202.

Mr. Bauermann is, or at least was, at the period his observations were made, a member of the Geological Survey of the British Islands, and therefore might be supposed to know what he was speaking about. I say so because though I have been able to confirm all his descriptions, yet it is satisfactory in a subject of controversy involving so many important matters to have the support of an additional qualified witness.

As already remarked, I cannot speak so confidently of Washington Territory, Oregon, and the interior of British Columbia to the east of the Cascade Mountains, being less familiar with that section of the Pacific slope. However, through western Oregon wherever I visited the country, down at least to the Umpqua river, and in Washington Territory to the very base of the Cascades, whatever further, I observed glacial remains not less marked than in the neighboring region of Vancouver Island. Some of the erratic blocks are scattered over the prairies of that region, standing on the stoneless grassy plains in marked contrast to their surroundings. These boul-

* "Tertiaries," Mr. Bauermann says, but if he refers to the Northern coal fields of the island, then there can be but little doubt that these beds are as I have given them, here and elsewhere, of secondary age.

ders and erratic blocks* have even attracted the notice of the Indians—otherwise so stolid in regard to the natural objects among which they live. One huge angular block, on the Snoqualami Prairie, has a tradition attached to it, to the effect that at one time it was suspended from the sky but was cast adrift to earth on account of the wrath of the Supreme Being, being roused at the licentiousness of a minor god and his myrmidons who for the time being were disporting themselves on it! Not far from the corner of the Peninsula of Saanich off the coast of Vancouver's Island, there are several large boulders—(apparently rounded by the waves and not by ice action?) which aboriginal tradition assert to have been some old witches turned into stone. My canoe-men in passing them used not unfrequently to stop there, and throw water on them, shouting: "Give us a wind, you old jade!" and as occasionally an afternoon breeze does not spring up in that region after the midday summer calms, the superstition obtained with them a semblance of belief, and so got handed on to posterity clothed in all the hoary sanctity of antiquity.

Groovings and other unequivocal marks of *general* ice action are not wanting in Washington Territory either. Even with the superficial glance we were enabled to give the subject in hurried journeys over that region, for other purposes, we observed not a few of such deep unmistakable ice planings. And in a note received recently from my friend and former traveling companion, Mr. Edmund T. Coleman, (well known as the author of the folio "Scenes from the Snow Fields of Mont Blanc," and who may therefore be supposed to know ice markings) he states, though with no view to combating the theory in hand, which indeed he knew nothing about:—

"I saw at Seahome (near Bellingham Bay) in the cuttings made for a tramway, the finest instances of fluting and grooving, evidences of glacial action, that I have ever seen on this coast. They were 90 feet in length, running N. and S. according to the theory of Professor Agassiz."

I have not been in Alaska proper, but in 1866 in a visit to the Queen Charlotte Islands lying some thirty or forty miles off the northern coast of British Columbia, close to the southern boundary of the former territory,† marks of the northern Drift quite as marked as in Vancouver's Island were found there.

Indeed in crossing the "spit" at the entrance of Skidegate

* By "boulders" I mean to designate rounded worn blocks of stone carried along in the *moraine profonde* of the ice sheet; by "erratic blocks," angular fragments of rock, apparently conveyed to their present resting place in ice (field ice or bergs) without having been subject to erosion. The necessity of the division is apparent. The first is very rare on the Pacific coasts and I suspect part of local moraines; the other is universal.

† I have described their geography in the Proceedings of the Royal Geographical Society of London, for 1869. Some remarks on their geology will likewise be found in the same place.

Sound, we had a dangerous reminder of the fact, having lost the keel of our schooner, on one of the great boulders which cover that locality. Masset Spit and other shoal localities are equally dangerous on that account. Here they present themselves disagreeably to the seaman's senses, but on *land*, though less visible on account of the dense vegetation concealing them, yet to one accustomed to search for such things, traveled blocks and ice groovings are sufficiently abundant. Boulder clay is also not wanting to complete the tale of the glacial period in Northwest America.

All throughout this paper I have sedulously avoided touching upon the modern local glaciers which are found scattered all throughout the northern portion of the Cascade and Coast Ranges of Mountains, in some places (as in some of the northern inlets on the coast of British Columbia) approaching to within a short distance of the sea; and in the southern part of the latter range they are found in most of the high mountains, such as Mt. Baker, Diamond Peak, etc. In another place, "On the formation of Fjords, etc."* I have shown that in all likelihood these British Columbian inlets were at one time the site of glaciers, and though the marks of local glaciers are evident here and there where none are now found, yet the appearances described are due to a totally different set of causes from these, or any now in existence on the American continent, unless indeed Greenland be included under that geographical division. These local glaciers in the limits assigned to a paper of this nature do not therefore require to be further touched upon.

Am I therefore in error, when I think that the case I have submitted, makes good the thesis with which I commenced these remarks, viz:—that whatever may be said of California and Alaska (and Messrs. Whitney and Dall are quite capable of holding their own in reference to their assertions about these regions), the Northern drift is certainly not absent from British Columbia, Vancouver's Island, Washington Territory and the Queen Charlotte Islands? With every respect to the observations of the gentlemen named, my more extended opportunities of investigation have, I think, enabled me to answer, with some degree of certainty, this question in the negative. Perhaps I would not have been so particular in discussing this question at length, had not Prof. Whitney's and Mr. Dall's idea been taken up in this country, and in America by geologists of no mean eminence,† and a disposition been shown by others less capable to build thereon theories, where no theories ought to be built.

4 Gladstone Terrace, Hope Park, Edinburgh, June 23d, 1870.

* Journal of the Royal Geographical Society, vol xxxix.

† Foster in "Mississippi Valley," p. 338, and A. Geikie in "Nature," vol. i, p. 436.

ART. XXXI.—*Extracts from the Address of* GEORGE BENTHAM, Esq., President of the Linnean Society, on the 20th of May, 1870.

IT had been my intention on the present occasion to carry on the sketches of the general progress of biological science which I had attempted in 1862, 1864, 1866, and 1868; but I have, from various causes, been unable to devote so much time as usual to the preparation of my Address, and feel obliged to confine myself to a few points connected with subjects of special interest to myself, which, within the last two or three years, have made considerable advances.

The most striking are, without doubt, the results obtained from the recent explorations of the deep-sea faunas, and from the investigation of the tertiary deposits of the arctic regions, which, although affecting two very different branches of natural science, I here couple together, as tending, both of them, to elucidate in a remarkable degree one of the most important among the disputed questions in biological history, the continuity of life through successive geological periods.

An excellent general sketch of the first discovery and progressive investigation of animal life at the bottom of the sea at great depths, up to the close of the season of 1868, is given by Dr. Carpenter in the Proceedings of the Royal Society, vol. xvii, No. 107, for Dec. 17, 1868. The results of the still more important expedition of the past year have as yet been only generally stated by Mr. Gwyn Jeffreys, in the numbers of 'Nature' for Dec. 2 and 9, 1869, and by Dr. Carpenter, in a lecture to the Royal Institution, published in the numbers of 'Scientific Opinion' for March 23 and 30 and April 6 and 13 of the present year; and further details, as to the Madreporaria, are given by Mr. Duncan in the Proceedings of the Royal Society, vol. xviii, No. 118, for March 24 of the present year; whilst, in North America, the chief conclusions to be drawn from these researches into the deep-sea fauna are clearly and concisely enumerated by Prof. Verrill, in the American Journal of Science for January last; and some of the more detailed reports of the American explorations, by Louis and Alexander Agassiz and others, have been published in the Bulletin of the Museum of Comparative Zoology at Harvard College, Nos. 6, 7, and 9 to 13. For the knowledge of the data furnished by the tertiary deposits of the arctic regions we are indebted almost exclusively to the acute observations and able elucidations of Prof. O. Heer, in his 'Flora Fossilis Arctica,' in his paper on the fossil plants collected by Mr. Whymper in North Greenland, published in the last part of the Philosophical Transactions for

1869, and in the as yet only short general sketch of the results of the Swedish Spitzbergen Expeditions, contained in the Genevese 'Bibliothèque Universelle, Archives Scientifiques,' for Dec. 1869.

It would be useless for me here to retrace, after Dr. Carpenter and Prof. Verrill, the outlines of the revolution which these marine discoveries have caused in the previouly conceived theories, both as to the geographical distribution of marine animals, and the relative influences upon it of temperature and depth, and as to the actual temperature of the deep seas, or to enter into any details of the enormous additions thus made to our knowledge of the diversities of organic life; and it would be still further from my province to consider the geological conclusions to be drawn from them. My object is more especially to point out how these respective dips into the early history of marine animals and of terrestrial forests have afforded the strongest evidence we have yet obtained, that apparently unlimited permanency and total change can go on side by side, without requiring for the latter any general catastrophe that should preclude the former.

There was a time, as we learn, when our chalk-cliffs, now high and dry, were being formed at the bottom of the sea by the gradual growth and decay of Globigerinæ and the animals that fed on them—amongst others, for instance, *Rhizocrinus* and *Terebratulina caput-serpentis;* and when, at a later period, the upheaval of the ground into an element where these animals could no longer live arrested their progress in that direction, they had already spread over an area sufficiently extensive for some part of their race to maintain itself undisturbed; and so, on from that time to the present day, by gradual dispersion or migration, in one direction or another, the same *Rhizocrinus* and *Terebratulina* have always been in possession of some genial locality, where they have continued from generation to generation, and still continue, with Globigerinæ and other animals, forming chalk at the bottom of the sea, unchanged in structural character, and rigidly conservative in habits and mode of life through the vast geological period they have witnessed. So also there was a time when the hill-sides of Greenland and Spitzbergen, now enveloped in never-melting ice and snows, were, under a genial climate, clothed with forests, in which flourished *Taxodium distichum* (with *Sequoiæ*, *Magnoliæ*, and when at a later period these forests were destroyed by the general refrigeration, the Taxodium already occupied an area extensive enough to include some districts in which it could still live and propagate; and whatever vicissitudes it may have met with in some parts, or even in the whole, of its original area, it has, by gradual extension and migration, always found some

spot where it has gone on and thriven, and continued its race from generation to generation down to the present day, unchanged in character and unmodified in its requirements. In both cases, the permanent animals of the deep-sea bottom and the permanent trees of the terrestrial forests have witnessed a more or less partial or complete change in the races amongst which they were commingled. Some of these primitive associates, not endowed with the same means of dispersion, and confined to their original areas, were extinguished by the geological or climatological changes, and replaced by other races amongst which the permanent ones had penetrated, or by new immigrants from other areas; others, again, had spread like the permanent ones, but were less fitted for the new conditions in which they had been placed, and in the course of successive generations had been gradually modified by the Darwinian process of natural selection, the survival of the fittest only among their descendants. If, in after times, the upheaved sea-bottom becomes again submerged, the frozen land becomes again suited for vegetation, they are again respectively covered with marine animals or vegetable life, derived from more or less adjacent regions, and more or less different from that which they originally supported, in proportion to the lapse of time and extent of physical changes which had intervened. Thus it is that we can perfectly agree with Mr. Duncan, that "this persistence (of type and species through ages, whilst their surroundings were changed over and over again) does not indicate that there have not been sufficient physical and biological changes during its lasting to alter the face of all things enough to give geologists the right of asserting the succession of several periods:" but we can, at the same time, feel that Dr. Carpenter is in one sense justified in the proposition, that we may be said to be still living in the Cretaceous period. The chalk formation has been going on over some part of the North Atlantic sea-bed, from its first commencement to the present day, in unbroken continuity and unchanged in character.

If once we admit as demonstrated the coëxistence of indefinite permanency and of gradual or rapid change in different races in the same area, and under the same physical conditions, according to their constitutional idiosyncracies, and also that one and the same race may be permanent or more or less changing, according to local, climatological, or other physical conditions in which it may be placed, we have removed one of the great obstacles to the investigation of the history of races, the apparent want of uniformity in the laws which regulate the succession of forms. We may not only trace, with more confidence, such modifications of race through successive geological periods as Prof. Huxley has recently exhibited to us in respect

of the Horse, but we can understand more readily the absolute identity of certain species of plants inhabiting widely dissevered areas, of which the great majority of species are more or less different. One of the arguments brought forward against the community of origin of representative species in distant regions, such as temperate Europe and the Australian Alps, the Arctic Circle and Antarctic America, the Eastern United States and Japan respectively—an argument which has long appeared to me to have considerable weight—was this:—if disseverance and subsequent isolation result necessarily in a gradual modification by natural selection, how is it that when all are subjected to the same influences, the descendants of some races have become almost generically distinct in the two regions, whilst others are universally acknowledged as congeners, but specifically distinct, and others again are only slight varieties or have remained absolutely identical? To this we can now reply, with some confidence, that there is no more absolute uniformity in the results of natural selection than in any other of the phenomena of life. External influences act differently upon different constitutions. Were we to remove the whole flora and fauna of a country to a distant region, or, what comes to the same thing, change the external conditions of that flora and fauna, as to climate, physical influences, natural enemies, or other causes of destruction, means of protection, &c., we should now be taught to expect that some of the individual races would at once perish; others, more or less affected, might continue through several generations, but with decreasing vigor, and, in the course of years or ages, gradually die out, to be replaced by more vigorous neighbors or invaders. Others, again, might see amongst their numerous and ever varying offspring some few slightly modified, so as to be better suited for the new order of things; and experience has repeatedly shown that the change once begun may go on increasing through successive generations and a permanent representative species may be formed. And some few races might find themselves quite as happy and vigorous under their new circumstances as under the old, and might go on as before, unchanged and unchanging.

Taking into consideration the new lights that have been thrown upon these subjects by the above investigations and by the numerous observations called forth by the development of the great Darwinian theories, amongst which I may include a few points adverted to in a paper on Cassia which I laid before you last year, but which a press of matter has prevented our yet sending to press, it appears to me that, in plants at least, we may almost watch, as it were, the process of specific change actually going on; or at least we may observe different races now living in different stages of progress, from the slight local

variation to the distinct species and genus. As the first step we may take, for instance, those races which are regarded by the majority of botanists as variable species, such as *Rubus fruticosus*, *Rosa canina*, *Zornia diphylla*, *Cassia mimosoides*, &c. We shall find in each some one form, which we call typical, generally prevalent over the greater part of the area of the race, whilst others, more or less aberrant, are more or less restricted to particular localities, the same varieties not occurring in disconnected stations with precisely the same combinations of character and in the same proportions; local and representative varieties and subspecies are being formed, but have not yet obtained sufficient advantages to prevent their being kept in check by their intercommunication (and, probably, cross-breeding) with their more robust type. The British Batologist or Rhodologist transported to the south of France or to Hungary will still find one, or perhaps two or three forms of Bramble and Dog-rose with which he is familiar; but if he wishes to discriminate the thirty or forty varieties or subspecies upon which he had spent so much labor and acuteness at home, he must recommence with a series of forms and combinations of characters quite new to him. The species is still the same; the varieties are changed. As examples of what we may call a second stage in the formation of species, we may adduce such plants as *Pelargonium australe* or *grossularioides* and *Nicotiana suaveolens* or *angustifolia*, to which I alluded in the above-mentioned paper on *Cassia*. Here we have one race, of no higher than specific grade in the ordinary acceptance of the term, inhabiting two countries which have long been widely dissevered (in the one case South Africa and Australia, in the other Chili and Australia), which, if originally introduced by accident from one country to the other, have been so at a time so remote as thoroughly to have acquired an indigenous character in both; in both they are widely spread and highly diversified: but among all their varieties one form only is identical in the two countries (*Pelargonium australe*, var. *erodioides*, and *P. grossularioides*, var. *anceps; Nicotiana suaveolens*, var. *angustifolia*, and *N. angustifolia*, var. *acuminata*), and that so comparatively a rare one that it may be regarded as being in the course of extinction; whilst all other varieties, some of them very numerous in individuals over extended areas, and all connected by nice gradations, diverge nevertheless in the two countries in different directions and with different combinations of characters, no two of them growing in the two countries being at all connected but through the medium of that one which is still common to both. When that shall have expired, the distinct species may be considered established. A still further advance in specific change is exem-

plified in *Cassia* itself, in which I have shown that no less than eight or nine different modifications of type, sectional and subsectional, are common to South America, tropical Africa, and Australia, but without any specific or, at least, subspecific identity, except perhaps in a few cases where a more modern interchange may be presumed. The original common specific types are extinct, the species have risen into sections. Common types of a still higher order have disappeared in the case of Proteaceæ, an order so perfectly natural and so clearly defined that we cannot refrain from speculating on the community of origin of the African and of the Australian races, both exceedingly numerous and reducible to definite groups—large and small well-marked genera in both countries, and yet not a single genus common to the two; not only the species, but the genera themselves have become geographical. As in the case of the varieties of *Pelargonium* and *Nicotiana*, so in that of the species of *Cassia* and of the genera of Proteaceæ, it is not to be denied that precisely similar modifications of character are observed in the two countries; but these modifications are differently combined, the changes in the organs are differently correlated. In Asiatico-African *Chamæcristæ* a tendency to a particular change in the venation of the leaflet is accompanied by a certain change in the petiolar gland; in America the same change in the gland is correlated with a different alteration in the venation. In Australian Proteaceæ the glands of the torus are constantly deficient with a certain inflorescence (cones with imbricate scales) which is always accompanied by them in Africa.

In selecting the above instance for illustration of what we may, without much strain upon the imagination, suppose to be cases of progressive change in races, it is not that they are isolated cases or exceptionally appropriate; for innumerable similar ones might be adduced. In the course of the detailed examination I have had successively to make of the floras of Europe, N. W. America, Tropical America, Tropical Africa, China, and Australia, I have everywhere observed that community of general type, in regions now dissevered, is, when once varied, accompanied by more or less of divergence in more special characters in different directions in the different countries.

With regard to the succession of races which have undergone a complete specific change through successive geological periods, we have not in plants, as far as I am aware, any such cases of "true linear types or forms which are intermediate between others because they stand in a direct genetic relation to them," as Professor Huxley appears to have made out in favor of the pedigree of the Horse in his last Anniversary Address to the Geological Society. And I may, in regard to plants, repeat with

still greater emphasis his dictum, that "it is no easy matter to find clear and unmistakable evidence of filiation among fossil animals; for in order that such evidence should be quite satisfactory, it is necessary that we should be acquainted with all the most important features of the organization of the animals which are supppsed to be thus related, and not merely with the fragments upon which the genera and species of the paleontologist are so often based." The difficulty is much greater in the case of fossil plants; for instead of bones, teeth, or shells, portions of internal or external skeletons, the parts preserved to us from the tertiary period are generally those least indicative of structural organization. Mr. Carruthers has recently (Geological Magazine, April and July 1869, and Journal of the Geological Society, August, 1869) adduced satisfactory evidence of the close affinity of *Sigillaria* and the allied genera of the coal-period with the living Lycopodiaceæ, formerly suggested by Dr. Hooker; but, as he informs me, no connecting links, no specimens, indeed, of the whole Order, have as yet been found in any of the intermediate cretaceous or tertiary deposits. Among the latter, the presence of numerous vegetable types, to which we may plausibly refer as to the ancestors of living races, is established upon unimpeachable data; but I have been unable to find that a single case of authentic pedigree, as successively altered from the cretaceous through the abundant deposits of the eocene and miocene period to the living races, has been as yet as satisfactorily made out as that of the absolute identity of *Taxodium* and others above mentioned, although I feel very little doubt that such a one will yet be traced when our paleontologists shall have ceased to confound and reason alike upon the best proved facts and the wildest guesses. Our late distinguished foreign Member, Professor Unger, whose loss we have so recently to deplore, had indeed, shortly before his death, published, under the name of 'Geologie der europäischen Waldbäume, part 1, Laubhölzer,' no less than twelve tabular pedigrees of European forest races; but it seems to me that in this, as in another of the same eminent paleontologist's papers to which I shall presently have to refer, his speculations have been deduced much more freely from conjectures than from facts. There is no doubt that the presence of closely allied representatives of our Beeches, Birches, Alders, Oaks, Limes, &c., in the tertiary deposits of Central and Southern Europe is fully proved by inflorescences and fruits as well as leaves; but how can we establish the successive changes of character in a race when we have only the inflorescence of one period, the fruit of another, and the leaf of a third? I do not find a single case in which all three have been found in more than one stage; and by far the great majority of these fossil species are

established on the authority of detached leaves or fragments of leaves alone.

Now let us consider for a moment what place a leaf really holds in systematic botany. Would any experienced systematic botanist, however acute, on the sole examination of an unknown leaf, presume to determine, not only its natural order and genus, but its precise characters as an unpublished species? It is true that monographists have sometimes published new species founded on specimens without flower or fruit, which from collateral circumstances of habitat, collector's notes, general resemblance, &c., they had good reason to believe really belonged to the genus they were occupied with; but then they had the advantage of ascertaining the general *facies* derived from insertion, relative position, presence or absence of stipular appendages, &c., besides the data supplied by the branch itself. And with all these aids, even the elder De Candolle, than whom no botanist was more sagacious in judging of a genus from general aspect, proved to have been in several instances far wrong in the genus, and even Order, to which he had attributed species described from leaf-specimens only. Paleontologists, on the other hand, have, in the majority of these tertiary deposits, had nothing to work upon but detached leaves or fragments of leaves, exhibiting only outward form, venation, and, to a certain degree, epidermal structure, all of which characters may be referred to that class which Professor Flower, in his introductory lecture at the Royal College of Surgeons in February last, has so aptly designated as *adaptive*, in contradistinction to essential and fundamental characters. They may, when taken in conjunction with relative individual abundance, assist in forming a general idea of the aspect of vegetation, and thus give some clue to certain physical conditions of the country; but they alone can afford no indication of genetic affinity, or consequently of origin or successive geographical distribution.

Lesquereux, in speaking of Cretaceous "species, or rather forms of leaves," observes, in a note to his paper on Fossil Plants from Nebraska (this Journal, vol. xlvi, July, 1868, p. 103), that "it is well understood that when the word *species* is used in an examination of fossil plants, it is not taken in its precise sense; for indeed no *species* can be established from leaves or mere fragments of leaves. But as paleontologists have to recognize these forms described and figured, to compare them and use them for reference, it is necessary to affix to them specific names, and therefore to consider them as species." But the investigators of the tertiary floras of Central and Southern Europe have acquired the habit, not only of neglecting this distinction and naming and treating these forms of leaves as species equivalent to those established on living plants, but of

founding upon them theories which must fall to the ground if such specific determination proves inaccurate. Nothing can be more satisfactory than such determinations as that of *Podogonium*, for instance, which Professor Heer has succeeded in proving, by numerous specimens of leaves, fruits, and even flowers, some of them still attached to the branches, which I had myself the pleasure of inspecting last summer under the friendly guidance of the distinguished Professor himself. This genus of Cæsalpinieæ, from its evident affinity with *Peltogyne*, *Tamarindus*, and others now scattered over the warmer regions of America and Africa, and more sparingly in Asia, tells a tale of much significance as to the physico-geographical relations of the Swiss tertiary vegetation, confirmed as it is by some other, equally or almost equally convincing examples. But the case appears to me to be far different with the theory so vividly expounded by Professor Unger in 1861 in his Address entitled "Neu Holland in Europa." This theory, now generally admitted, seems to me to be established on some such reasoning as this:—There are in the tertiary deposits in Europe, and especially in the earlier ones, a number of leaves that look like those of Proteaceæ are a distinguished feature in Australian vegetation; *ergo*, European vegetation had in those times much of an Australian type derived from a direct land communication with that distant region.

This conviction, that Proteaceæ belonging to Australian genera were numerous in Europe in Eocene times, is indeed regarded by paleontologists as one of the best-proved of their facts. They enumerate nearly one hundred tertiary species, and most of them with such absolute confidence that it would seem the height of presumption for so inexperienced a paleontologist as myself to express any doubt on the subject. And yet, although the remains of the tertiary vegetation are far too scanty to assert that Protaeceæ did not form part of it, I have no hesitation in stating that I do not believe that a single specimen has been found that a modern systematic botanist would admit to be Proteaceous unless it had been received from a country where Proteaceæ were otherwise known to exist. And, on other grounds, I should be most unwilling to believe that any of the great Australian branches of the Order ever reached Europe. As this is a statement requiring much more than mere assertion on my part, I shall beg to enter into some detail, commencing with a short summary of my grounds of disbelief in European tertiary Proteaceæ, and then examining into the supposed evidences of their existence.

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From the above considerations, I cannot resist the opinion that all presumptive evidence is against European Proteaceæ,

and that all direct evidence adduced in their favor has broken down upon cross-examination. And however much these Eocene leaves may assume a general character, which may be more frequent in Australia (in Proteaceæ and other Orders) than elsewhere, all that this would prove would be, not any genetic affinity with Australian races, but some similarity of causes producing similarity of adaptive characters.

Another series of conclusions drawn by paleontologists from their recent discoveries, which appears to me to have been carried too far, relates to the region where a given species originated. The theory that every race (whether species or group of species derived from a single one) originated in a single individual, and consequently in one spot, from which it has gradually spread, is a necessary consequence of the adoption of Darwinian views; and when Mr. R. Brown ("On the Geographical Distribution of Conifers," Trans. Bot. Soc. Edin., x, p. 195) sneers at my having qualified it as a perfect delusion, he must have totally misunderstood, or rather misread the passage he refers to in my last year's Address. The expression is there specially applied to the idea of general centers of creation whence the whole flora of a region has gradually spread, in contradistinction to the presumed origin of individual races in a single spot, which is there as distinctly admitted. The determination of where that spot is for any individual race, is a far more complicated question than either geographical botanists or paleontologists seem to suppose. "Every vegetable species," as well observed by Professor Heer, "has its separate history," and requires a very careful comparison of all the conclusions deducible as well from present distribution as from ancient remains. The very important fact that *Taxodium distichum*, *Sequoiæ*, *Magnoliæ*, *Salisburia*, &c., existed in Spitsbergen in Miocene times, so satisfactorily proved by Heer, shows that the vegetation of that country then comprised species and genera now characteristic of North America; but it appears to me that the only conclusion to be drawn (independently of climate and geology) is, that the area of these species and genera had extended continuously from the one country to the other, either at some one time, or during successive periods. The proposition that "Spitsbergen appears to have been the focus of distribution of *Taxodium distichum*," because an accidental preservation of its remains shows that it existed there in the lower-Miocene period, would require at least to be in some measure confirmed by a knowledge of the flora of the same and preceding periods over the remainder of its present area, the greater part of which flora, however, is totally annihilated and forever concealed from us. The fact that *Pinus Abies* existed in Spitsbergen in Miocene times, and that no trace of it has been found in the abundant Tertiary remains

of Central Europe is very instructive. It might show that that tree was of more recent introduction into the latter than the former country; but it cannot prove that it was not still earlier in some other region, whence it may have spread successively into both territories, still less that its course of dissemination was directly from Spitsbergen over Northern and Central Europe. Moreover the determination of *Pinus Abies* is not so convincing as that of the *Taxodium*, resting as it does, if I correctly understand Prof. Heer's expression, on detached seeds and leaves, with a few scales of one cone, and may require further confirmation.

In the above observations it is very far from my wish to detract from the great value of Professor Heer's researches. Interested as I have been in the investigation of the history of races of plants, I have deeply felt my general ignorance of paleontology, and consequent want of means of checking any conclusions I may have drawn from present vegetation by any knowledge of that which preceded it, and the impossibility at my time of life of entering into any detailed course of study of fossils. Like many other recent botanists, I am obliged to avail myself of the general results of the labors of paleontologists; and if I have here ventured on a few criticisms, it is only as a justification of the hope that they may in some measure distinguish proved facts from vague guesses, in order that we may know how far reliance is to be placed on their conclusions. * * * * * *

ART. XXXII.—*Account of the fall of a Meteoric Stone in Stewart County, Georgia;* by Professor JOSEPH E. WILLET.

IN October, 1869, I learned that a metoric explosionh ad occurred in Stewart county, Georgia. I immediately requested Hon. John T. Clarke, a resident of the county adjoining Stewart, to enquire whether any stone or stones had fallen, and to endeavor to procure them for Mercer University. Judge Clarke, after considerable labor, was entirely successful in his search; and, through him, Mr. Barlow, in whose yard the meteorite descended, generously presented it to our Museum. To Judge Clarke and to Mr. Latimer, I am indebted for the following history of the phenomena attending the descent of the meteorite.

Mr. J. B. Latimer of Bladen's creek, Stewart county, has kindly furnished the following particulars of the *flight* of the body through the air, and of the several *explosions*, which occurred nearly vertically above him.

"The morning of the 6th October last (1869) was quite clear,

scarcely any cloud being visible, quite calm; about 10 A. M. the atmosphere grew somewhat hazy, no clouds; at about 15 or 20 minutes before 12 M. a roaring, rushing sound was heard in a northwesterly direction, about 80 degrees above the horizon. In a moment or two, it was almost directly over head, at which point a loud explosion occurred, followed in rapid succession by six other reports, but less in volume than the first—making seven in all. The explosions appeared about as loud as a 12-pound cannon, at a distance of 10 or 12 miles. These explosions did not occur all at the same point in the heavens, but seemed to emanate from some body moving rapidly to the south-east. After the explosions, a peculiar whirring sound was heard, apparently produced by some large irregular body, moving very rapidly. This also went in a southeasterly direction. This sound was heard several seconds; many have compared it, and aptly too, to an imperfect steam-whistle. I have no precise idea of the time consumed in all this demonstration; some persons say several minutes—but I think 10 or 15 seconds would about cover the time.

"As the larger body was going out of our hearing, (some moments after the explosions) a smaller one passed to the south-west, with just such a noise as is always produced by a flying fragment of a shell after its explosion, or of any angular body cast violently through the air. This piece descended to the earth, distinctly traced in its passage by many persons, and struck in the yard of Capt. E. Barlow—which point of contact is, on an air-line, about 2½ miles from a perpendicular beneath where the explosions occurred. This is the only one known to have fallen in this section.

"The explosions, together with the rushing sound afterward, were heard over a region about 30 miles N.E. and S.W. and 50 or 60 miles N.W. and S.E. No shock was felt—at least no tremor of the earth.

"Two men say, that they were looking in the exact direction of the explosions at the time they occurred, and saw a quantity of vapor, much like the volume of steam escaping from the pipe of an engine, at each successive stroke; which vapor or mist was violently agitated, and increased in bulk, with each successive report, but disappeared soon after the cessation of the reports. This corroborates the testimony of some of my own laborers, who say, that immediately after the explosions something like a thin cloud cast its shadow over the field they were in."

Hon. John T. Clarke, of Cuthbert, Ga., who has interested himself in collecting the history of the meteorite, and through whose influence it has come into the possession of Mercer University, writes me the following particulars of its *fall*.

"It fell about 11½ A. M. on the 6th of October last, (1869), in Stewart county, Ga., on the premises of Elbridge Barlow, Esq., about 12 miles south of west from Lumpkin. Capt. Barlow picked it up a few moments after it fell. His account of it is this. While standing in the open yard, the sky being bright and clear, he heard first a succession of about three explosions, resembling sudden bursts of thunder, or discharges of artillery, followed by a deep roaring for several seconds, and then by a rushing or whizzing sound of something rushing with great speed through the air near by. The sound ceased suddenly. The noise, from first to last, was some half a minute. Two negroes were washing near the well, in the same yard, about sixty yards from where Barlow stood. They heard the noise, and supposed it to be the falling in of the plank well-curbing, banging from side to side in its descent, and so spoke of it to one another before it fell. While they were speaking thus, it struck the ground about twenty steps from them, in full sight, knocking up the dirt. They called Capt. B. and showed him the spot. It was upon very hard trodden ground in the clean open yard. The earth was freshly loosened up very fine in a circle of about one and a half feet in diameter; and, upon scraping the loose dirt away with the hands, the stone was found about ten inches below the surface. From the direction in which the ground was crushed in, it must have come from the northwest, and at an angle of about 30 degrees with the horizon. The stone when picked up was covered all over with the black shell which it bears now, except a triangular spot on one corner, about one inch each way, where the corner appeared freshly knocked off, and about four other spots near a quarter of an inch in diameter, where the shell was slightly knocked off. The other bruises, which you will find upon it, have been made since by persons who have handled it. To enable you to distinguish the original breaks upon it, I have marked each of them with a red cross. The stone still has a strong odor, which I will not undertake to describe. Capt. B. says it smelled stronger when he first picked it up. He does not remember that it had any noticeable heat. It was not cold, as a stone found so deep in the ground should be.

"The stone weighs now 12¼ ounces, about ½ ounce has been pecked off from it. Its color within is strikingly like very light granite; and, with the exceptions above noted, it is entirely covered with a smooth, almost black shell, a trifle thicker than common letter paper, so that externally it looks very much like a lump of iron ore. It is an irregular, seven-sided figure, its longest side being about 2⅜ inches long. If put into a spherical form, it would make a ball about 1⅜ inches in

diameter. So far as I have been able to ascertain, no other parts have been found.

"The noise attending this phenomenon is variously described by different persons, and from different places. Two intelligent ladies residing four miles south of Lumpkin, nearly east of where the stone fell, and about ten or twelve miles off, describe it thus. While sitting in the house they heard, as it were, the sound of a great fire suddenly bursting forth from some confinement into the open air. They rushed out of doors, and heard the roaring sound continue for several seconds. They located the source of the noise in the direction of Barlow's.

"In Cuthbert, about 18 miles from Barlow's, nearly southeast, a gentleman, engaged in a workshop, heard a lumbering noise, which he took to be several heavy pieces of machinery in an adjoining room, falling down one after another. On going in, he found no one, and that he had mistaken the cause of the noise. Many persons here heard sounds like repeated thunder followed by roaring. Some say that they first heard several rapid, cracking explosions, like that of volleys of small arms, followed immediately by the louder burst of artillery. Most persons here thought the noise came from the southeast, passed over the place in a northwesterly direction, and died away in the distant northwest.

"The foregoing statements have been selected from many in circulation, showing how differently the senses were affected at different points. The facts are purposely presented in their nakedness. If you can find them available in aid of a scientific investigation of the origin of this phenomenon, I shall have accomplished more than I expect."

The above accounts agree as to the main facts. They were furnished by Mr. Latimer and Judge Clarke, without being compared by them. It is possible that a comparison of notes by them might have thrown some light on the point of greatest discrepancy, viz: the direction of flight. It is probable that the meteorite came from some point in the *north* quarter; the statement of Mr. Latimer over whom it exploded, and that of Mr. Barlow as to the direction in which the earth was penetrated, concur in this regard. Persons in Cuthbert, who represent it as coming from the south, may have been misled by an echo, mistaking this for the original sound.

Prof. J. Lawrence Smith, who is giving special attention to the subject of meteorites, has requested the privilege of analyzing the stone above described.

Mercer University, Penfield, Georgia.

ART. XXXIII.—*Description and Analysis of a Meteoric Stone that fell in Stewart county, Ga. (Stewart county Meteorite), on the 6th of October*, 1869; by J. LAWRENCE SMITH.

IN October, 1869, I learned through the public press that certain meteoric phenomena had occurred in Stewart county, Georgia, and that one or more stones had fallen. Enquiries were immediately instituted by me, and, through Prof. Willet, I obtained for examination the only stone found, one that was seen to strike the ground, and from him received an account of the phenomena observed at the time by Messrs. Latimer, Clarke and others. [See preceding Article.]

The stone, as it reached me, was nearly intact, and weighed 12¼ ounces; it must originally have weighed 12½ ounces. It is of an irregular conical shape, having a flattened base, and is covered with a dull heavy black coating. The specific gravity is 3·65. The fractured surface has a grayish aspect, and when examined closely, especially by the aid of a glass, exhibits numerous greenish globules with a whitish granular material between; through the mass are dark particles consisting principally of nickeliferous iron, with some pyrites, and a few specks of chrome iron. The nodules are sometimes three or more millimeters in diameter, and of an obscure fibrous crystalline structure, the crystals radiating usually from one side of the nodule; they have a dirty bottle-green color, a greasy aspect when broken, and are more or less opake.

Some of these little nodules were separated in a tolerable state of purity, amounting to 121 milligrams; on analysis they afforded:

		Oxygen.	Ratio.
Silica	48·62	25·90	2
Alumina	8·05	3·79	
Protoxyd of iron	11·21	2·51	1
Magnesia	30·18	11·80	
	98·06		

The hardness of the mineral is about 6, and it is quite tough. The formula would be $\dot{R}Si$, with a part of the silica replaced by alumina, a not unfrequent case in minerals such as hornblende, hypersthene, &c. As it is impossible to derive any light from its crystalline structure, the above analysis warrants me in concluding that it is either *bronzite*, or *hornblende*, but I am more inclined to the former supposition as it appears to take the place of the enstatite in many meteorites.

Nickeliferous iron constitutes about 7 per cent of the mass,

and a portion separated in as pure a state as possible, afforded on analysis—

Iron	86·92
Nickel	12·01
Cobalt	0·75
	99·68

These are the proportions after allowing iron for a small amount of sulphur, present in a minute quantity in the nickeliferous iron, which could not be separated mechanically. I did not test for copper or phosphorus; the quantity of iron separated from the stone did not warrant my making special analyses for substances, the quantity of which present could only be exceedingly minute.

The stony matter freed from the iron was treated with nitro-muriatic acid and water, and heated for some time over a water bath, renewing the water and acid once or twice; the solution was filtered, and the residue washed; the residue was then treated with a warm solution of caustic potash, filtered and again washed. The filtrate was neutralized by hydrochloric acid, and added to the first filtrate, and the whole evaporated to dryness over a water bath, warmed gently over the lamp, and treated with water and a little hydrochloric acid, thrown on a filter, the silica collected and estimated; the last filtrate was treated with a solution of hydrochlorate of baryta to ascertain the quantity of sulphuric acid present, (due to the pyrites in the original mass); it was found to indicate 6·10 per cent of magnetic iron pyrites. The solution freed from the excess of baryta was now analyzed in the ordinary way.

The insoluble portion of the meteorite was fused with carbonate of soda and a small fragment of caustic potash, and its ingredients ascertained.

A separate portion of the stony part of the meteorite was examined for alkalies.

The various analyses referred to above gave—omitting the nickeliferous iron:

The part soluble in acid	58·05
" " insoluble "	41·95

	Soluble part.	Insoluble part.
Silica	41·08	56·03
Alumina	0·32	5·89
Protoxyd of iron	18·45	15·21
Magnesia	41·06	21·00
Lime	----	0·10
Soda, with a little K and Li	----	2·97
	100·83	101·20

The soluble part consists principally of olivine. The insoluble is doubtless the bronzite already refer to, with a little albite or oligoclase.

Chrome iron was detected by fusing some of the stony part of the meteorite with carbonate of soda and a little niter, and separating in the usual way. The quantity was quite minute.

The composition of the stone as made out would be

Nickeliferous iron	7·00
Magnetic pyrites	6·10
Bronzite, or hornblende Olivine Albite, or oligoclase Chrome iron	86·90
	100·00

ART. XXXIV.—*Some practical remarks on the use of Flame Heat in the Chemical Laboratory, especially that from burning gas without the aid of a blast;* by J. LAWRENCE SMITH, Louisville, Ky.

THERE is probably no more important era in the operations of the chemical laboratory than that of the introduction of the lamp as a source of heat for a large number of chemical operations, and that without the aid of a blast. Berzelius was doubtless the first to accomplish much in this direction, which he did by the agency of the lamp that so commonly bears his name, and which, more or less modified, is still in use where the ordinary illuminating gas is not to be had.

Although illuminating gas has been in use for about seventy years, it is only within a comparatively recent date that it has been pressed into service, and used as a heating agent in the laboratory. The reason of this arose from the fact that when burnt in the ordinary manner it deposited soot on the vessels heated by it. This difficulty has been overcome by burning the gas from small orifices made in a tube bent in the form of a circle, the holes being from 1 to 2 centimeters apart, and, sometimes, combining two or more rings in concentric circles. This method, however, has not been generally adopted.

We must date the successful introduction of gas for heating purposes to the use of a mixture of gas and air passed through wire gauze and ignited above the gauze, giving a flame without light and with great heat; the invention of this method is

claimed by several, and doubtless was discovered by different individuals at about the same time, without a previous knowledge of each other's results; this method is still more or less employed for certain purposes.

The next step in this direction, and doubtless the most important up to the present time, is to burn the mixture of gas and air without the agency of wire gauze; it was first made known to the public in the burner commonly called the Bunsen burner, doubtless from its being either invented or brought into extensive practical use by the distinguished chemist of Heidelberg. Its form is too well known to require more than a mere mention here, and it is now made of all sizes from those capable of burning 4 cubic feet of gas and under, to those which can burn 15 or 20 cubic feet from a single burner, or from a combination of several smaller ones. To this burner, some material additions have been made by different individuals. J. J. Griffin, (the chemical instrument dealer in London), was, I believe, the first to introduce the use of the rosette and the register for the supply of air. The most remarkable results accomplished by this method of burning gas and air are those obtained by G. Gore of Birmingham, (all of whose results I have verified), where gold, copper, cast iron, &c., were fused in crucibles without the agency of any artificial blast. Mr. Gore evidently realized fully the true principle of burning this mixture, so as to obtain a maximum effect; the burner, however, with its furnace arrangements, is unavoidably of a form and on a scale limiting its application.

The usual form of the Bunsen burner, with the rosette and register (when required), bids fair to hold its own against any other form for general purposes, and whatever modifications may be made on it should be of such a character as not to entrench on its simplicity. One or two of these modifications are now in daily use in my laboratory, for which there is no claim to any special originality, nor are they intended to supplant the ordinary form.

As simple an instrument as the Bunsen burner appears to be, its principles and effects are well worthy of being carefully studied.

As the gas passes from the small orifices* in the lower part of the burner, and mixes with the air drawn in at the lower opening, and passes out at the open end of the tube, it usually contains not quite enough oxygen for its complete combustion, and requires free access of air to the outer portion of the flame

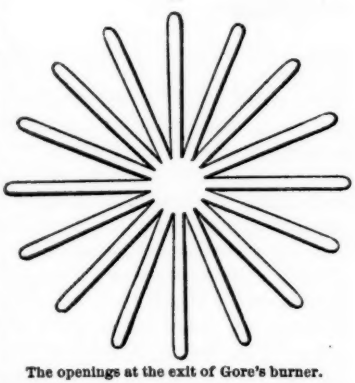
* The outlet for gas may be in the form of crossed slits or two small holes of (1-32 inch diameter each) for the small size burner, the length of tube being about 4 to 4½ inches long; the next larger has four openings (about 1-25 inch diameter each) and the tube about 5 inches long.

to complete the combustion; yet even with this, the flame is hollow in its lower portion, having a cool center, its most intense heat being at about three or four inches above the end of the tube in the smaller Bunsen-burners, and eight or ten inches in the largest size. If a proper access of air is not allowed to the flame, as sometimes happens in some of the furnace connections occasionally used with Bunsen's burner, acetylene is formed from the imperfect combustion, which is recognized by its disagreeable odor, or by collecting some of the gas formed during the combustion; the presence of acetylene may be rendered evident by a small amount of a solution of ammoniacal cuprous chlorid.

The best heating effects of the gas used in the ordinary round Bunsen burner, when employed in the heating of crucibles and other vessels, are not obtained; yet in the great majority of cases the small loss of gas is not worth considering, especially as to obtain better results in most cases, would only complicate this beautifully simple instrument.

To get the best effects of heat, we must imitate the principle applied in the Argand burner, namely to flatten down the exit of the mixed gases. It was by following out this principle that Mr. Gore was enabled to make a burner having a number of radial flat orifices as represented in the figure (1), the air from without having free access to the flame along the entire length of the slit openings, the number of slits used are more numerous than those represented in the figure. With the flame from this burner introduced into a certain form of refractory cylinders, cast iron can be melted in a crucible, without the aid of a blast, as has already been stated; the little chimney to the furnace being two inches in diameter, and four feet long. This burner and its furnace is of but limited application, and the amount of gas consumed considerable.

1.



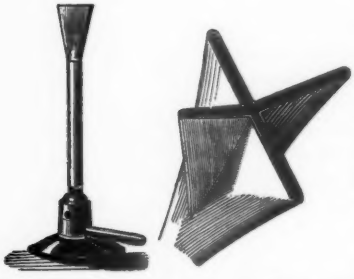
The openings at the exit of Gore's burner.

The principle, however, of the above burner is introduced in constructing a more simple form, and the flattened orifice is now used in the construction of what I conceive to be the best form of furnace for heating glass tubes for organic analyses and other purposes; such furnaces are made by Weisnig of Paris, and Desaga of Heidelberg.

The use of the flattened burner is not fully appreciated; its advantages are, that there is no cold point in the flame, and the burner can be brought much nearer to the object to be heated, within 20 to 25 millimeters for the small sized burners. In this burner as usually made, the opening is too broad, experience having convinced me that a slit 2 millimeters across and about 40 millimeters in length is the most effective one for a small size burner, consuming about 5½ cubic feet per hour; this burner is represented in fig. 1, which can be used with the ordinary tube, by detaching the tube with the flattened orifice.

By taking a burner of this description and putting two pieces on each side of the center, as represented in fig. 2, a very efficient burner is made for heating platinum crucibles in silica fusions, &c., and with such a burner, consuming 5½ to 6 cubic feet of gas per hour, I conduct most effectually all silica fusions in one hour or less, taking care to protect the crucible from the current of the air by a properly constructed short conical chimney, which chimney can be made of soap stone, sheet iron, or any other convenient material.

2. 3.



As was stated in the commencement of this article, it was not intended to describe the more complicated methods of burning gas in furnaces and by means of a blast, but to confine the remarks to the simpler forms in every day use, which can be made to accomplish all the requirements of the usual laboratory operations, and when a higher heat is required, the furnace must be our recourse, whether burning gas, charcoal or coke. The burner represented in figure 2 is the one I now employ in heating the crucible in my method of alkali determination with carbonate of lime and sal ammoniac, which method, with its more recent modifications, will be published in a very short time. The description of it, with all the minute details of manipulation, being ready for the press.

ART. XXXV.—*On the connection between Terrestrial Temperature and Solar Spots;* by CLEVELAND ABBE, Director of the Cincinnati Observatory.

WE are indebted to Sir Wm. Herschel for the suggestion that probably the presence of numerous spots on the surface of the sun is indicative of increased chemical activity, and is accompanied by increased radiation of heat. The investigations and theories of the past ten years however would lead us to an opposite conclusion from that of Herschel.

Immediately on the receipt of the Astronomische Nachrichten containing Wolf's tabular view of the relative frequency of the solar spots for the past three centuries, I made an extended comparison of the numbers therein given with such meteorological tables as were then accessible to me. After much labor I was forced to conclude that the variations of solar heat are so slight that they are masked in the local climatic peculiarites.

On further reflection, however, it seemed certain that the heat radiated from a dark spot should be of low intensity, and would therefore be largely absorbed by the aqueous vapor of our own atmosphere as well as by that of the sun. I have therefore been lately led to make a special study of the series of observations made on the Hohenpeissenberg, and published in the supplementary volume I. of the Annals of the Munich Observatory.* This series specially deserves attention because of the remarkable uniformity of the circumstances under which the observations were made; it extends from 1792 to 1850, omitting the years 1793, 1799, 1811, 1812 and 1817.

Assuming that the number of visible solar spots or groups are an index of the existing solar radation of heat, we have but to compare the number (s) expressing the relative spot frequency as given by Wolf with the mean annual temperature, (t_1) as given by Lamont. The solution of the equation $t_1 = t_0 + s\tau$ gives us t_0 and τ, which latter is the coefficient of solar spot influence on the radiation of heat.

The accompanying table exhibits for each year the value of s and t_1,—the latter expressed in degrees of Reaumur.

The arithmetical mean of the annual temperatures gives

$$M_1 = +5°{\cdot}178 \pm 0°{\cdot}061$$

prob. error of one annual mean $= \pm 0°{\cdot}449$

The residuals are given in the column $t_1 - m_1$.

Introducing the term $s\tau$ we find by the method of least squares

$$(t_1) = +5°{\cdot}450(\pm 0°{\cdot}086) - s \times 0°{\cdot}00789(\pm 0°{\cdot}00204)$$

p. e. of one annual mean $= \pm 0°{\cdot}430$.

* Volume VII, containing the continuation of this series has not yet been received.

The residuals are given in the column $t_1-(t_1)$.

Year.	*s.*	t_1	t_1-m_1	$t_1-(t_1)$	t_2	t_2-m_2	$t_2-(t_2)$
		°	°	°	°	°	°
1792*	53	5·38	+0·20	+0·38	+7·11	+0·28	+0·43
94	24	6·16	+0·98	+0·90	7·77	+0·94	+0·85
95	16	5·66	+0·48	+0·34	7·32	+0·49	+0·34
96	9	5·36	+0·18	−0·02	6·90	+0·07	−0·14
97	6	6·06	+0·88	+0·66	7·77	+0·94	+0·71
98*	3	5·30	+0·12	−0·13	7·61	+0·78	+0·53
1800	10	6·24	+1·06	+0·87	8·68	+1·85	+1·65
01	31	5·56	+0·38	+0·35	7·23	+0·40	+0·37
02	38	4·64	−0·54	−0·51	7·50	+0·67	+0·70
03	50	4·84	−0·34	−0·22	6·56	−0·27	−0·15
04	70	5·31	+0·13	+0·41	6·87	+0·04	+0·32
05	50	4·09	−1·09	−0·97	5·67	−1·16	−1·04
06	30	6·24	+1·06	+1·03	7·54	+0·71	+0·67
07	10	5·87	+0·69	+0·50	7·44	+0·61	+0·41
08	2	4·90	−0·28	−0·53	6·47	−0·36	−0·62
09	1	6·09	+0·91	+0·65	6·80	−0·03	−0·30
10*	0	6·56	+1·38	+1·11	8·20	+1·37	+1·09
13	14	4·39	−0·79	−0·95	6·07	−0·76	−0·93
14	20	4·34	−0·84	−0·95	6·16	−0·67	−0·79
15	35	4·05	−1·13	−1·12	6·15	−0·68	−0·68
16*	46	3·66	−1·52	−1·43	5·31	−1·52	−1·43
18	34	5·57	+0·39	+0·39	7·30	+0·47	+0·46
19	22	5·58	+0·40	+0·30	7·30	+0·47	+0·27
20	9	4·38	−0·80	−1·00	6·21	−0·62	−0·83
21	4	5·42	+0·24	0·00	7·07	+0·24	−0·01
22	3	6·37	−1·19	+0·94	8·09	+1·26	+1·01
23	1	4·82	−0·36	−0·62	6·67	−0·16	−0·43
24	7	5·33	+0·15	−0·06	6·85	+0·02	−0·20
25	17	5·27	+0·09	−0·05	7·00	+0·17	+0·03
26	29	5·15	−0·03	−0·07	6·80	−0·03	−0·08
27	40	5·06	−0·12	−0·07	6·15	−0·68	−0·63
28	52	5·46	+0·28	+0·42	7·04	+0·21	+0·35
29	54	3·99	−1·19	−1·03	5·06	−1·77	−1·62
30	59	5·00	−0·18	+0·02	6·30	−0·53	−0·34
31	39	5·39	+0·21	+0·25	6·87	+0·04	+0·07
32	22	4·96	−0·22	−0·32	6·70	−0·13	−0·23
33	8	5·17	−0·01	−0·22	6·07	−0·66	−0·97
34	11	5·99	+0·81	+0·63	7·88	+1·05	+0·86
35	46	4·69	−0·49	−0·40	6·26	−0·57	−0·48
36	97	4·98	−0·20	+0·30	6·51	−0·32	−0·18
37	111	4·23	−0·95	−0·34	5·74	−1·09	−0·48
38	83	4·20	−0·98	−0·60	5·76	−1·07	−0·68
39	68	5·10	−0·08	+0·20	6·82	−·01	+0·26
40	52	4·38	−0·80	−0·66	5·96	−·87	−0·73
41	30	5·60	+0·42	+0·39	7·37	+·54	+0·50
42	20	5·09	−0·09	−0·20	6·80	−·3	−0·15
43	9	5·24	+0·06	−0·14	6·79	−·4	−0·25
44	13	4·67	−0·51	−0·68	6·32	−·51	−0·66
45	33	4·87	−0·31	−0·32	6·58	−·25	−0·26
46	47	6·24	+1·06	+1·16	7·87	+1·04	+1·14
47	79	5·05	−0·13	+0·22	6·76	−·7	+0·28
48	100	5·62	+0·44	+0·96	7·36	+·53	+1·05
49	96	5·27	+0·09	+0·58	7·03	+·20	+0·69
1850	64	4·76	−0·42	−0·19	6·45	−·38	−0·15

That the probable errors are on the whole very little diminished, is owing to the presence of a few large discordances; on the other hand, the small probable error of the coefficient of s would indicate that it has a real existence.

As the daily 2 P. M. observation may be supposed to show with special clearness the direct heating power of the sun, I have sought for a confirmation of the preceding results by applying the same formula to the annual means of the temperatures observed at this hour.

The annual mean temperature at 2 P. M. is given for each year in the column t_2 of the accompanying table.

The arithmetical mean gives

$$M_2 = +6°{\cdot}830 \pm 0°{\cdot}067$$

p. e. of one annual mean $= \pm 0°{\cdot}489$

The solution for the co-efficient of s gives

$$(t_2) = +7°{\cdot}108(\pm 0°{\cdot}100) - s \times 0°{\cdot}00801(\pm 0°{\cdot}00221)$$

p. e. of one annual mean $= \pm 0{\cdot}465$.

This result therefore corroborates the former in indicating a decrease in the amount of heat received from the sun during the prevalence of spots—a result clearly in harmony with the recent investigations into the nature of the solar photosphere.

The reality of the existence of the above coefficient of s will be rendered more striking to the eye if the mean of several years' observations is taken at the period of maximum and minimum spot frequency.

It would be interesting to seek in the above residuals for evidence of other temperature periods than that dependent on the eleven year spot period. There are indeed plain indications of such a period of about fifty or fifty-five years duration—probably identical with Wolf's fifty-six year period—but our series of observations is not extended enough to justify any exact conclusion.

If we acknowledge the probability of a connection between planetary configurations and solar spots, then we are at once led to make a direct connection between the former and the temperature variations. Such an investigation I have begun and the indications are that positive results will be attained, and such as will demonstrate that the solar spots are but an imperfect index to the periodic changes in the solar radiation; these periodic changes being apparently more intimately and directly connected with the tides in the cool atmosphere surrounding the solar photosphere. The results of this investigation will be made known so soon as the recent observations on the Hohenpeissenberg can be incorporated into the work.

Cincinnati, July 20, 1870.

ART. XXXVI.—*On a new method of determining the Level-error of the axis of a meridian instrument;* by C. A. YOUNG, Ph.D., Professor of Natural Philosophy and Astronomy in Dartmouth College.

[Read at the Troy meeting of the Am. Association for the Advancement of Science]

THE inclination of the axis of a meridian instrument to a horizontal plane has hitherto been measured by three different methods: by the use of the spirit level; by examining with a collimating eye-piece the image of the wires as in nadir-point observations, the collimation having been previously determined either by reversal of the instrument or by collimators; and lastly by observing the transits of stars by reflection from an artificial horizon.

The first of these methods is by far the most used, and with portable instruments is sufficiently convenient. Still it requires a good deal of time, and, in the case of a large instrument, of hard work; and if there are sensible irregularities upon the pivots of the instrument it is a very troublesome operation to ascertain and apply the necessary corrections.

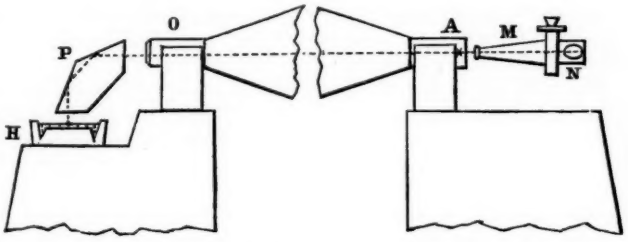
The second and third methods are still more laborious: the second gives the level error corresponding to but one single position of the telescope, i. e., with the telescope pointing downward, and is therefore liable to a constant error depending upon any malformation of the pivots which affects the instrument in this particular position: the third method can be used only when the air is perfectly still.

The method I have to propose, allows the determination of this error without any further labor than two readings of a microscope, in any position of the telescope, and without that uncomfortable climbing which is involved in the use of the striding level or nadir observations.

The annexed diagram illustrates the arrangement of the apparatus, in which, however, no regard is paid to the relative proportion of parts; the prism and mercurial horizon being grossly exaggerated in size for the sake of distinctness.

The axis of the instrument is to be fitted up as a collimator in the same manner already practised by Challis, Airy and others. In place of cross-wires, however, the extremity A should be provided with a plate of thin glass having a minute dot or circle engraved upon it, the plate being adjustable so that this dot can be brought into the geometrical axis of revolution and into the focus of the small object-glass which is situated in the other pivot, O. A reading microscope, M, is attached to the pier and provided with an ordinary collimating

eye-piece N, by which light can be thrown upon the dot through the tube of the microscope. This enables us to measure the



vertical distance between the dot and its image formed by reflection in the manner to be described.

Opposite O is fixed a prism shaped like that of a camera lucida, which by *two* total reflections bends the light through a right angle. Immediately below it is placed a mercurial horizon. If an ordinary right angled prism were employed, producing the bend by a *single* reflection, then any disturbance of the prism would disturb twice as much the relation between the ray passing from A to O and that returning; but with a prism of the form proposed this relation is independent of any small changes in the position of the prism. Distortion of the prism, which is hardly to be feared, could alone do any harm.

A prism of this form and mercurial horizon, thus combined, form in effect a vertical plane mirror, whose verticality is independent of any small instability of the pier upon which it is mounted.

It is then easy to see that if the dot be accurately centered and if the axis is level the image of the dot will exactly coincide with the dot itself, provided that the reflecting angle of the prism be exactly 135°. If, however, the angle vary slightly from this the image of the dot will fall above or below the dot itself by a small amount, which will be constant, and can be determined once for all by any one of several different methods. Any deviation of the axis from horizontality will immediately be indicated by a change in this distance twice as large as the deviation itself, and may be accurately measured by the microscope. Any inaccuracy in the centering of the dot is immediately eliminated by taking two measurements in opposite positions of the telescope.

The mercurial horizon employed should of course be so devised as to be free from tremors as far as possible. The form recently described by J. H. Lane, of the U. S. Coast Survey, appears to leave little to be desired in this respect.

There is no difficulty in arranging the apparatus so that the ordinary illumination of the wires at the eye-piece of the teles-

cope shall be effected by the light transmitted through the body of the microscope. So arranged, the apparatus remains in constant readiness for use, and, as before remarked, requires only the labor of taking two microscope readings for each determination of level error, without involving any disturbance of the setting of the instrument.

I may add in closing, that this virtual mirror of constant inclination to the horizon may easily find other applications,—as for instance in determining the horizontal points of a vertical circle where the object glass of the instrument is not so large as to require a too unwieldy and expensive prism.

Hanover, N. H. Aug. 1, 1870.

ART. XXXVII.—*Influence of Temperature on the modulus of Elasticity of certain Metals;* by F. KOHLRAUSCH, Ph.D., and FRANCIS E. LOOMIS, Ph.D.

[Communicated in extracts to the Academy of Sciences, Göttingen, May 7, 1870.]

THE results obtained by Wertheim,* in his investigations respecting the influence of temperature on elasticity, have thus far generally served as a basis for calculation. It will be observed in comparing these data, that for certain substances, especially for iron, the singular phenomenon manifests itself that for different temperatures the variations of the modulus undergo a change of sign. From 0° C. to 100° the modulus increases, and then from 100° to 200° decreases. This increase is certainly altogether contrary to what would naturally have been expected. Such a maximum, however, is still more remarkable. In view, therefore, of the uncertainty of the results of investigations thus far made known, and the importance of an accurate knowledge of the variations of the modulus of elasticity for different temperatures in many of the finer measurements, the present investigations were undertaken with a view if possible of obtaining more reliable results for some of the metals of greatest practical utility. For practical purposes it would suffice to determine the variations of elasticity within the limits of the more ordinarily occurring temperatures. This was partially accomplished by Kupffer, whose investigations we shall refer to hereafter. Nevertheless in consequence of the results obtained by Wertheim, it appeared of especial interest to determine again the variations in regard to their uniformity, as well as in regard to a change of sign. The observations were extended therefore to high temperatures, in general from 90° to 20° C., and in one case very nearly to 0°. Within these limits

* Pogg. Ann. Erg.. Band 2, S. 61; Ann. de Chimie, 3me S, T. 12, p. 443.

the variations in the elasticity can be determined from the observations with great accuracy. Since they show a remarkable uniformity, there can be no hesitation in assuming the resulting empirical formula as very nearly accurate, considerably beyond these limits.

It is scarcely necessary to observe that the investigations of Wertheim have afforded the most valuable material for a knowledge of the coefficient of elasticity. His method of observation, however, is little adapted to afford a solution of the question here under consideration. For Wertheim determined the absolute moduli of elasticity by means of the dilatation of bars and wires at different temperatures. The difficulties besetting an absolute determination of the variations of quantities amounting in all to only about $1\frac{1}{2}^{mm}$ are obvious. They were especially serious for the higher temperatures, since the imperfect heating apparatus of Wertheim could not be kept at a constant temperature for any considerable period of time, and, in consequence, the variations in length due to the fluctuations of temperature, inevitably exercised a serious influence on those arising from a change in elasticity. Wertheim himself designated these investigations as not rigorously accurate.* It would unquestionably have been more rational to confine the absolute determinations to ordinary temperatures, and to determine the influence of the temperature by means of other methods which here as in other cases, where it is a question merely of variations, are capable of a much greater sensibility. The influence of the fluctuations of the temperature on the length of the object will always occasion serious inconvenience in investigations on dilatation.

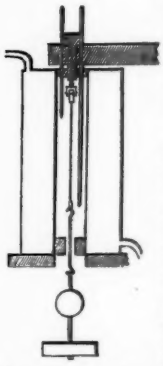
All these difficulties disappear, however, and at the same time the most accurate method of observation is obtained, by employing for investigation the torsion elasticity, whose choice is further to be recommended from the fact that torsion is so generally employed in measurements. If a wire is loaded with a weight, and set in vibration about its vertical axis, the reciprocal value of the square of the time of vibration affords a direct measure for the coefficient of the torsion of the wire. Since observations of the period of vibration are among the most accurate known in physics, the variations of elasticity may be thus determined with all the rigor desirable.

1. *Apparatus and mode of observation.*—The following dispositions were adopted for the practical application of this method. (See fig. in about $\frac{1}{1\frac{1}{2}}$ natural size). The vibrating body consisted of a cylindrical leaden weight with vertical axis. Above it, attached to a brass rod, is a mirror for the purpose of observation with telescope and scale. The wire is clamped at each end

* Ann. de Chimie, 3me S., T. 12, p. 400 and 401.

between brass plates, of which the lower pair could be united to the weight by means of a upsilon rest, and the upper pair in like manner to a brass rod screwed into a wooden pivot. The latter was so secured in a solid support fixed to the wall as to be capable of rotation. The torsion vibrations were communicated to the weight by revolving the pivot by means of a lever. (Omitted in the figure).

2.



The space within which are enclosed the wire and a very considerable length of the connecting pieces is the hollow core of a double cylinder of tin, so constructed that the space inclosed between the two cylinders could be heated. For the determination of the temperature, three thermometers were prepared, such that the quicksilver reservoirs are at different distances from the zero point. When the graduated tubes project from the apparatus from the division 0°, the quicksilver reservoir of one of the thermometers is at a height corresponding to the middle of the wire and close beside it. The other two quicksilver reservoirs are at each end of the wire, and arranged symmetrically around it. The two latter thermometers alone are represented in the figure. The readings were made by means of a telescope.

The tin cylinder was tightly inclosed on all sides with a covering of felt of about a centimeter in thickness. It stood upon a support attached to the wall. The weight and mirror were contained in a box closed in front with a glass slide. These details are omitted in the figure.

It was originally designed to fill the vessel with water at different temperatures, but a simpler method, at first only employed to test what results might be expected, proved better adapted to the end in view. The felt envelop renders the loss of heat so gradual, that the empty apparatus, previously heated with steam, cools with sufficient slowness to permit the period of vibration at different temperatures to be observed with accuracy during the process. Accordingly the apparatus was heated for a series of observations by the admission of steam, until the temperature became tolerably stationary. The steam connection was then interrupted, and the temperature and vibrations alternately observed. Thus the variations of the elasticity of a wire for different temperatures are obtained in the course of a few hours. This simple mode of procedure affords at the same time the advantage of leaving the apparatus untouched from the beginning to the end of a series of observations, excepting only a slight turning of the wooden pivot, when the amplitude of the vibrations becomes too small. Several days

were required for hot water to assume the temperature of the room, and it is not advisable to compare together observations made at long intervals, since the longer the interval the greater also becomes the danger that the conditions of the observation change in a manner to exercise on the period of vibration an influence feeble indeed, yet perhaps sensible, in view of the slight extent of the variations under consideration. For the same reason there are objections to employing water at different temperatures, since its renewal is scarcely possible without jarring the apparatus.

2. *Reduction of the observations.*—In the calculation of the data obtained from observations made with gradually diminishing temperatures, certain corrections are necessary, which render the reductions somewhat complicated. The manner in which the calculation was conducted, can be best explained by means of an example in detail. It is relative to a copper wire.

Each period of vibration was obtained according to the well known method of Gauss from two times of elongation, of which each depended in this case on six observations of the passage of the spider line through the position of rest. These times of elongation are contained in the first column of Table I. (See below). In the second column is contained the number of vibrations transpired between these times; in the third the period of vibration obtained by the division of the intervals in column one, by the numbers in column two.

TABLE I.

Times of elongation.			Number of vibrations.	Period of vibration.	Temperature		
					Observed.	Corrections.	Corrected.
h	m	s		sec.	°	°	°
10	54	29·93					
			10	20·9970	79·4	−3·06	76·34
	57	59·90					
			9	20·9667	75·2	−2·95	72·25
11	1	8·60					
			9	20·9422	71·8	−2·83	68·97
	4	17·08					
			24	20·9000	66·4	−2·61	63·79
	12	38·68					
			29	20·8558	58·8	−2·28	56·52
	22	43·50					
			25	20·8248	52·7	−2·04	50·66
	31	24·12					
			39	20·7849	46·7	−1·78	44·92
	41	54·73					
			36	20·7614	41·3	−1·56	39·74
	57	22·14					
			58	20·7352	36·4	−1·36	35·04
12	17	24·78					
			92	20·7065	31·2	−1·15	30·05
	49	9·78					
			96	20·6861	27·0	−1·00	26·00
1	22	15·65					
			15	20·6767	25·2	−0·92	24·28
	27	25·80					

The temperatures, column 4, corresponding to the several periods of vibration, were obtained by a graphical representation of all the observed temperatures. The arithmetical mean was first taken of each three simultaneous readings of the thermometers, and then traced on coördinate paper, with the time of its observation as abscissa. The temperature for any moment can then be obtained from the curve, with an accuracy of about 0°·2 for the higher and about 0°·1 for the lower temperatures. The numbers in the 4th column are these mean temper-

atures for the several periods of vibration, i. e. they give the reading of the thermometers for the mean moment between the beginning and end elongation. When the interval of time is so considerable that the corresponding curve cannot be regarded as a straight line, the temperature τ would be thus obtained somewhat too low. To determine the true value, the arithmetical mean τ_0 was taken from the temperatures which correspond to the beginning and end time of elongation. The correct temperature is then $\tau + \frac{\tau_0 - \tau}{3}$.

Correction of the readings of the thermometers.—The numbers in the 4th column give the true temperature of the wire on condition, 1st, that the thermometers are accurate; 2d, that their temperature for each instant is the same as that of the space in which they hang. The heavier connecting pieces attached to each extremity of the wire extend so far within the heated space that the difference of temperature which might result in the wire from conduction may be neglected. It may be assumed, therefore, that the very fine wire follows sensibly the temperature of the space within which it is suspended. Since finally the readings of the upper and lower thermometers differed from the mean of all three in the highest temperatures only some 2°·5, the mean can be regarded as the temperature of the wire, without sensible error.

The above two conditions, however, must be fulfilled, or since they are not fulfilled, the readings must be corrected.

1. The three thermometers were therefore first calibred, and their fixed points determined. Thence tables were constructed containing the corrections with regard to a quicksilver thermometer theoretically accurate. The three tables united in one, permit then to apply the correction directly to the mean value of the three readings.

2. Secondly, in consequence of the considerable mass of quicksilver, the thermometers do not follow the temperature of the space so completely as can be assumed of the fine wire. Now according to well known laws, the amount of heat lost by a body, and consequently therefore also the variation of its temperature, is proportional to the difference of its temperature and that of the surrounding space. Since in the present case, the velocity of the variation of temperature of the thermometers is known for each moment, it is only necessary to determine once for all the relation of the true temperature to that indicated by the thermometers, to be able to calculate the difference, i. e. the correction of the readings. For this determination a very delicate thermometer was employed, whose cylindrical quicksilver reservoir possessed a diameter of only some 3^{mm}, and contained in all less than 1·5 gr. of quicksilver. It

was assumed that this thermometer follows the gradually diminishing temperature of the air to within a negligable fraction, and the three thermometers were compared with it, under conditions similar to those existing in the observations on elasticity. All three thermometers were disposed at an equal height with the thermometer of comparison, in the gradually diminishing temperature of the hollow space of the tin cylinder, and simultaneous readings were made. They were also compared together in the ordinary manner in water of different temperatures. From these observations, which were conducted by Mr. Grotrian, it resulted that the mean of the readings of the three thermometers remained 2°·02 behind the temperature of the thermometer of comparison, when the velocity of the diminishing temperature amounted to 1° in 1 min. The correction of the readings due to the mass of quicksilver is therefore

$$2{\cdot}02\,\frac{d\tau}{dt},$$

where τ denotes the temperature of the thermometers, and t the time expressed in minutes. We obtain thus the temperatures which would have been observed, if thermometers had been employed containing the same amount of quicksilver as the thermometer here employed for comparison.

3. Finally a *third correction* is necessitated by the fact that the column of quicksilver of the thermometers, beginning with the zero point of graduation, possessed a lower temperature in consequence of projecting outside of the apparatus. There exists as yet no simple method of determining the mean temperature of the projecting column. It was assumed to have possessed the temperature of the air to the extent of three parts, and that of the interior of the apparatus one part. If τ_0 denotes the temperature of the room, the correction of the reading τ is therefore $-\frac{3}{4}\tau(\tau-\tau_0)\,0{,}00016$.

This value cannot be far removed from the truth. It coincides with the difference which the determination of the boiling point afforded, according as the thermometers were placed in the steam only to the zero point of graduation, or to the boiling point. The uncertainty of this assumption is at all events of small importance, since the whole correction for the highest temperature (90°) amounts only to 0°·8.

Thus, three corrections are to be applied to the readings of the thermometers. The first, which relates to the ordinary errors, is a function of the temperature; the second, in consequence of the mass of the quicksilver, is a function of the velocity of the diminution of the temperature; the third, finally, in consequence of the column of quicksilver projecting from the apparatus, depends on the difference of the outer and inner

temperatures. Nearly the same conditions prevailed, however, during all the series of observations. The temperature of the room differed from the mean at most 5°, and consequently the velocity of the diminution of the temperature of the apparatus depends, within negligable differences, on the inner temperature alone, as is confirmed indeed by the observations themselves. Thus the true velocity of the diminution for different temperatures could be determined from a few series of observations, whence by multiplication with 2·02, (see p. 355) the correction itself is obtained as function of the temperature.

In like manner, for the calculation of the third correction in the formula (p. 355), it was permissible to take $\tau_0 = 20°$ as a mean for all the observations.

The corrections may now be very much simplified by uniting all three in tabular form, viz: for reading,

0°	10°	20°	30°	40°	50°	60°	70°	80°	90°

the correction is

0°·0*	−0°·4	−0°·7	−1°·1	−1°·5	−1°·9	−2°·3	−2°·8	−3°·1	−3°·2

The interpolation for intermediate temperatures was accomplished by means of a graphical representation of this table. In one case, however, when the apparatus was filled with water, and the velocity of refrigeration could be neglected, the corrections 1 and 3 were applied separately.

Correction of the observed periods of vibration.—1. In the present investigations we are seeking the variations of the modulus of elasticity caused by a change of temperature, whereas we observe the directive force of the whole wire, which, in consequence of the heating, experiences a dilatation,as well as a change in its elasticity. Since this directive force of the wire depends on the length and section as well as on the modulus of elasticity, it is necessary to investigate what corrections are thereby necessitated.

It is an interesting fact that no correction at all is necessary. The dilatation in length and breadth completely neutralize each other.

If we denote by

l the length of the wire,
r the radius,
m the mass of the unity of length,
K the moment of inertia of the vibrating weight,
t the time of vibration,

* It is merely an accidental coincidence, that there is no correction to be applied to the reading 0°. The mean of the zero points of the three thermometers happens to be perfectly accurate.

D the directive force of the wire, i. e. the moment of revolution which it exercises for a torsion angle of $\frac{180^\circ}{\pi} = 57^\circ{\cdot}296$.

ε the modulus of torsion of the substance constituting the wire, we have

$$D = \frac{\pi^2}{t^2} K$$

and

$$\varepsilon = \frac{Dl}{gmr^2} = \frac{\pi^2 K}{g} \frac{l}{mr^2 t^2}, \qquad (1)$$

where g denotes the acceleration due to gravity, and ε is the number, the quintuple of which (according to the theory of Poisson, i. e. when the ratio of the transversal contraction to the longitudinal dilatation is $=\frac{1}{4}$) multiplied by g gives the square of the velocity of sound in the substance.

When now in consequence of a change of temperature, the elasticity and dimensions of the wire vary, the time of vibration t changes also. If we denote the variations of ε, l, m, r and t, occurring for a small change of temperature, by $d\varepsilon$, dl, dm, dr and dt, we obtain by differentiating (1) logarithmically,

$$\frac{d\varepsilon}{\varepsilon} = \frac{dl}{l} - \frac{dm}{m} - \frac{2dr}{r} - \frac{2dt}{t}.$$

But on the supposition that the wire experiences an equal dilatation in all directions on account of the temperature $\frac{dl}{l} = \frac{dr}{r}$. Also since m denotes the mass of the unity of length, obviously

$$-\frac{dm}{m} = \frac{dl}{l} = \frac{dr}{r}.*$$

Therefore

$$\frac{d\varepsilon}{\varepsilon} = -2\frac{dt}{t}. \qquad (2)$$

The modulus of torsion ε is therefore without correction reciprocally proportional to the square of the time of vibration.

The definition of the modulus of elasticity assumed here as a basis, denotes, with regard to the dilatation, the weight which must be suspended to a wire, whose unity of length is the same as the unity of its mass, to double the length. In practice it is customary, though frequently less convenient, to employ the section instead of the weight of the unity of length. In such case, the above expression (1) must be multiplied further by the density of the substance, and the coefficient of the cubical dilatation 3α must be subtracted from formula (2), so that

$$\frac{d\varepsilon}{\varepsilon} = -\frac{2dt}{t} - 3\alpha.$$

* $m.l = (m+dm)(l+dl)$

$0 = l\,dm + m\,dl$

$\frac{dm}{m} = -\frac{dl}{l}$.

2. A small correction is necessary when the moment of inertia of the vibrating weight changes in consequence of fluctuations in the temperature. In general it is inconsiderable in the same series of observations. The temperature in the box containing the vibrating weight was observed from time to time, and the period of vibration corrected when changes of temperature occurred. This was accomplished by multiplying the observed time of vibration by $1-0{,}00003.\Delta\tau$, where 0,00003 denotes the coefficient of the linear dilatation of lead, and $\Delta\tau$ the variation of temperature.

3. Theoretically the time of torsional vibrations should be independent of the amplitude. In fact in the present observations on the brass wire, no influence was perceptible. The iron wire, hereafter to be discussed, was especially investigated with respect to such an influence, however, and showed a manifest, though very small, increase of the time of vibration for an increment of amplitude. The increase appeared to be proportional to the latter, and on this supposition amounted to $0{\cdot}000037^{\text{sec.}}$ or $\frac{1}{5000}$ of the whole time for one division of the scale ($\frac{1}{7000}$ to one degree of arc). A series of observations on the copper wire were incidentally of a nature to afford the means of calculating the increase approximately. It amounted to $0{\cdot}000015^{\text{sec.}}$ for one division of the scale. The times of vibration were reduced by means of these numbers to an infinitely small amplitude.

Although it would be of interest to examine this phenomenon more minutely, it hardly enters within the scope of the object in view in these observations. For in consequence of the inconsiderable amplitudes which we employed (at most 200 divisions of the scale, or some 3°) the correction in extreme cases amounted only to $0{\cdot}005^{\text{sec.}}$, and it is almost completely compensated in its influence on the final result.

3. *Calculation of the coefficient of temperature.*—We have thus far obtained a series of temperatures with the corresponding times of vibration. From each two pairs of corresponding values may be readily derived the variation of the elasticity for 1°. We express it as a function of the total elasticity at 0°. If ε_0 designates the modulus of elasticity at 0°, and ε_1, ε_2, the moduli corresponding to the temperatures τ_1, τ_2, the desired decrease for 1° is found from formula (1) to be

$$\frac{1}{\varepsilon_0}\cdot\frac{\varepsilon_1-\varepsilon_2}{\tau_2-\tau_1} = t_0^2\,\frac{\dfrac{1}{t_1^2}-\dfrac{1}{t_2^2}}{\tau_2-\tau_1},$$

where t_1 and t_2 designate the times of vibration for the temperatures τ_1 and τ_2, and their squares are inversely proportional to the modulus of elasticity.

The following Table 2 contains in the second column the preceding diminutions of the modulus of elasticity for 1° taken from Table 1, by combining in it each two adjoining values. The time of vibration t_0 for 0° was obtained from the graphical representation of the observations by prolongation of the curve, a method which affords sufficient accuracy. Thus it was found that $t_0 = 20{\cdot}58$ sec. In the first column is contained the mean of the two temperatures at which the times of vibration were observed.

TABLE 2.

Temperature.	Decrease of the coefficient of elasticity for 1° as func. of the coeff. of elast. at 0°. Observed.	Calculated.	Observ.–Calc.
74°	0,00068	0,00068	±0,00000
71	69	67	+ 2
66	75	65	+ 10
60	56	62	− 6
54	50	60	− 10
48	65	57	+ 8
42	43	55	− 12
37	53	53	± 0
33	55	51	+ 4
28	48	49	− 1
25	0,00052	0,00047	+0,00005

The numbers of the second column can be regarded as the values of $-\frac{1}{\varepsilon_0}\frac{d\varepsilon}{d\tau}$ for the temperature τ of the first column. It will be observed that this mode of observation affords the decrease of the modulus of elasticity for 1° with tolerable accuracy, even for the times of vibration for two temperatures differing only by a few degrees. (The greatest difference is 7°, the smallest 2°). This decrease for the brass wire is noticeably greater for the higher temperatures than for the lower.

The numerical law of the variation is to be derived from all the observations. We seek to represent it by the formula

$$\varepsilon = \varepsilon_0 (1 - a\tau - b\tau^2).$$

It would obviously be more in accordance with the laws of the calculus of errors to determine the values of the coefficients ε_0, a and b by means of least squares from all the observed values of ε or $\frac{1}{t^2}$ for all temperatures. This would prove, however, when applied to all the series of observations, excessively laborious. It would be hazardous to unite previously together all the observations made on the same wire, since the time of vibration for the same temperature appears to be subject to slight variations from one day to another, probably in consequence of the considerable heating to which the wire is subjected.

By differentiating the above equation we obtain

$$-\frac{1}{\varepsilon_0}\frac{d\varepsilon}{d\tau} = a + 2b\tau.$$

The left hand expression denotes the values contained in the second column of Table 2. If therefore we substitute these values in the above equation, the coefficients a and b can be readily calculated.

By means of least squares there results for the brass wire

$$a = 0{,}000368 \qquad 2b = 0{,}00000425$$

by means of which the figures in the third column were calculated. In view of the small differences of temperature the accordance must be regarded as satisfactory. It appears still more manifest when the time of vibration itself is calculated. Since a and b are known, the most probable value of the time of vibration for 0° may also be readily determined by means of least squares. The calculation gives $t_0 = 20{\cdot}5696^{\text{sec.}}$

The times of vibration calculated from this value of t_0, together with the times observed, are given in the following Table 3. The difference between the observed and calculated values of the times of vibration amounts at most to $\frac{1}{1\,2\,5\,0}$ of a second, and indicates sufficiently the admissibility of the simplified method of reduction, although the regularity in the sign of the differences would seem to indicate that the rigorous method would afford a still greater accordance.

TABLE 3.

Temperature.	Times of vibration. Observed.	Calculated.	Observ.—Calcul.
76°·34	20ˢ·9970	20ˢ·9986	—0ˢ·0016
72·25	20·9667	20·9680	—0·0013
68·97	20·9422	20·9443	—0·0021
63·79	20·9000	20·9078	—0·0078
56·52	20·8559	20·8592	—0·0033
50·66	20·8248	20·8218	+0·0030
44·92	20·7849	20·7869	—0·0020
39·74	20·7614	20·7569	+0·0045
35·04	20·7352	20·7309	+0·0043
30·05	20·7065	20·7041	+0·0024
26·00	20·6861	20·6837	+0·0024
24·28	20·6767	20·6751	+0·0016

4. *Temperature and modulus of elasticity of iron, copper and brass.*—The method of observation and reduction, already described and applied to an example, was employed for wires of iron, of copper and of brass. All three wires were hard drawn and of a diameter between 0·2 and 0·3ᵐᵐ. The copper wire was composed of copper precipitated electrolytically. The iron

and brass wires are the ordinary wires of commerce wound on spools.

To abbreviate the calculation, the times of vibration and the temperatures of each series of observations are united first in groups by taking the mean, and especial care was taken to allow about the same weight to all the several coefficients to be derived. The observed data corrected according to the rules of chapter 2, are contained in the following Table 4, in which the Roman characters denote the chronological succession of the several series of observations.

TABLE 4.

	Iron.			Brass.			Copper.	
	Temp.	Time of Vibration.		Temp.	Time of Vibration.		Temp.	Time of Vibration.
		s.			s.			s.
I	76°34	17·6154	I	69°01	20.9412	I	74°06	12·4444
	56·87	17·5282		47·95	20.8067		47·96	12·3476
				28·85	20.7012		55·20	12·3696
II	76·55	17·5950					40·80	12·3221
	55·75	17·5087	II	72·14	20.9105		24·04	12·2667
	42·42	17·4553		48·78	20.7600			
	32·11	17·4150				II	62·60	12·3852
	21·91	17·3679	III	70·28	20·8694		22·05	12·2465
				49·11	20·7368			
III	78·73	17·5920		36·24	20·6669	III	77·27	12.4303
	59·72	17·5130		27·23	20·6193		56·16	12·3546
	20·97	17·3583					40·79	12·3028
			IV	50·05	20·7333		23·37	12·2413
IV	49·57	17·4715		42·81	20·6915			
	37·80	17·4279		34·59	20·6425			
	21·95	17·3627						
			V	19·43	20·5428			
				5·05	20·4737			

The following Table 5, in which the numbers are arranged according to the temperatures, is obtained by reducing the values contained in Table 4 in the same manner as those of Table 2 of the example.

The coefficients a and b are derived by means of least squares from the values contained in the third and fourth columns of Table 5, as in the example. Designating by ε_0 the modulus of elasticity at 0°, the modulus for the temperature τ is found to be,

for iron, $\varepsilon = \varepsilon_0(1 - 0{\cdot}000447\tau - 0{\cdot}00000012\tau^2)$
for copper, $\varepsilon = \varepsilon_0(1 - 0{\cdot}000520\tau - 0{\cdot}00000028\tau^2)$
for brass, $\varepsilon = \varepsilon_0(1 - 0{\cdot}000428\tau - 0{\cdot}00000136\tau^2)$

The numbers of the next to the last column of Table 5 were calculated with these values. The probable error of each value of the decrease of the coefficient of elasticity for 1° for a given temperature calculated from two groups of observations amounts accordingly to $\pm$ 0·000014.

TABLE 5.

		Temp.	Decrease of the coeff. of elastic. for an incr. of temp. of 1° as func. of the coeff of elast. at 0°. Observed.	Calcul.	Observ. — Cal.
Iron.	IV	29°·9	0·000467	0·000454	+ 0·000013
	II	32·2	482	455	+ 27
	III	40·3	450	457	— 07
	IV	43·7	416	458	— 42
	II	43·9	445	458	— 13
	II	66·1	459	463	— 04
	I	66·6	494	463	+ 31
	III	66·9	459	464	— 05
Copper.	III	32·1	0·000566	0·000538	+ 0·000028
	I	32·4	528	538	— 10
	II	42·3	542	544	— 02
	I	48·0	521	547	— 26
	III	48·5	531	547	— 16
	I	61·0	578	554	+ 24
	III	66·7	556	557	— 01
Brass.	V	12·2	0·000466	0·000461	+ 0·000005
	III	31·7	504	514	— 10
	I	38·4	523	532	— 09
	IV	38·7	570	533	+ 37
	III	42·7	514	544	— 30
	IV	46·4	556	554	+ 02
	I	58·5	593	587	+ 06
	III	59·7	584	590	— 06
	II	60·5	599	592	+ 07

If the other definition of the modulus of elasticity is employed (as was done by Wertheim) referring to the section, and not, as above, to the mass of the unity of the length of the wire, the coefficients of the temperature will be slightly altered in accordance with the observation on page 357. If the coefficients of dilatation for 1° are assumed to be for iron = 0·000012; for copper = 0·0000175; and for brass = 0·000019, the coefficients of $\varkappa$ become,

for iron,	0·000483,
for copper,	0·000572,
for brass,	0·000485.

The coefficients of the quadratic terms remain sensibly unchanged.

It is evident from the results of these observations that the mean variation of the elasticity for the three metals investigated differs but little from that of the temperature. For a change of temperature from 0° to 100° the diminution is for

Iron,	4·6	and	5·0	per cent.
Copper,	5·5	"	6·0	"
Brass,	5·6	"	6·2	"

where the numbers of the second column relate to the second definition of the modulus of elasticity.

If the variation in elasticity is compared with the influence exercised by the temperature on other properties of bodies it will be remarked that it is much greater than the cubical dilatation as well as the variations of refraction. It is of about the same order as the variation of the permanent magnetism caused by the temperature, as well as that of specific heat. The increase of the galvanic resistance on the other hand is much greater.

It results further from the sign of the quadratic term that the variation of elasticity for all three metals is the most rapid in the higher temperatures. While, however, the increase for iron is almost imperceptible, and is also very small for copper, it is quite considerable for brass. The decrease for 1° is for

	At 0°.	At 50°.	At 100°.
Iron,	0·0447	0·0459	0·0472 per cent.
Copper,	0·0520	0·0548	0·0576 "
Brass,	0·0428	0·0564	0·0699 "

It will be observed that the differences in the variation of the coefficient of elasticity for the three metals investigated, are in the order of the height of their melting points.

The results of these investigations show no trace at all of the remarkable phenomenon of a maximum, alluded to at the beginning of this article, which would seem to be indicated for iron by the investigations of Wertheim. If, therefore, different varieties of iron do not manifest a totally different behavior, or if the modulus of the longitudinal elasticity does not undergo a very different change in consequence of temperature from that of the torsional elasticity, this anomaly must be accounted for by the imperfect method of observation employed by Wertheim. This supposition is confirmed further by the observations of Kupffer (see below), as well as by a very simple experiment. If, namely, two tuning forks are in vibration, and one of them is heated, the number of vibrations changes in the manner demanded by the assumption of a decrease of elasticity for increasing temperatures.

It is to be remarked here that Wertheim's calculation of the heat generated by condensation during vibration, loses thus all its value.*

Observations of Kupffer.†—These investigations appear to be much less generally known than they deserve, for they contain much varied and valuable material for practical use. Kupffer's observations are in general on bars vibrating trans-

* Pogg. Ann., Bd. 11, S. 32.

† Mem. de l'Acad. de St. Petersb. 1856, 6 ser. T. vi, p. 400.

versally at the temperatures of about $-10°$, $+15°$ and $+80°$. Unfortunately, however, very long intervals (at least a year) occur between the observations, and it would seem that he did not always employ the same bars, so that his investigations permit but a limited conclusion in regard to the law of the variations of elasticity as modified by temperature. Finally, the dilatation caused by temperature was disregarded in the numerical values given by Kupffer, a source of serious errors in view of the method of observation which was employed.

A few of the results derived by Kupffer were obtained by means of torsional vibrations. Among these are the coefficients of temperature for the elasticity of an iron wire (piano cord), of a copper wire and of a brass wire. After having applied to these values the correction required for the dilatation of the vibrating disc, the decrease of the elasticity for 1° expressed in parts of the total elasticity is found to be

for	iron,	0·00055	("mem." S. 446)
"	copper,	0·00082	S. 464
"	brass,	0·00039	(S. 467)

These values agree tolerably well with those found by us, with the exception of the copper wire, in regard to which it is possible that the divergence may be caused by the circumstance that we employed a chemically pure metal.

5. *The absolute moduli of elasticity of the iron, copper and brass wires calculated from torsional vibrations and from the velocity of sound.*

To determine from our investigations the absolute moduli of torsional elasticity, it is necessary to know the dimensions and mass of the wires, as well as the moment of inertia of the vibrating weight.

The latter is to be calculated from the mass of the perforated lead cylinder $=1818$ grs. and the interior radius $=0{\cdot}34$ c. m. and the exterior radius $=5{\cdot}07$ c. m. It is found to be

$$\frac{1818}{2}\left(\overline{5{\cdot}07}^2+\overline{0{\cdot}34}^2\right)=23470 \text{ grs. } \square \text{ c. m.}$$

To this must be added the moment of inertia $=40$ grs. $\square$ c. m. of the connecting pieces and the mirror, which was calculated from their size and form. The total moment of inertia therefore is $K=23510$ grs. $\square$ c. m.

Further was determined

	Iron.	Copper.	Brass.
The length of the wire,	$l=20{\cdot}80$	20·75	20·80 c. m.
The mass of one c. m. length,	$m=0{\cdot}00301$	0·00655	0·00403 grs.
The density,	$\Delta=7{\cdot}82$	9·00	8·41
The time of vibration at 20°,	$t=17{\cdot}35$	12·23	20·55 sec.
The radius of the wire,	$r=0{\cdot}0111$	0·0152	0·0123 c. m.

The modulus of torsion T is derived from these data by means of the formula, p. 357,

$$T=\frac{\pi^2 K}{g}\frac{l}{mr^2t^2}.$$

It is found to be

for iron =444 kilometers.
" copper=217 "
" brass =190 "

If it is desired to follow the definition customary in practice, and to refer the modulus to the section instead of to the mass of the unity of length, these numbers must be multiplied by the corresponding densities; whereby they become

for iron, 3470 $\frac{\text{kilogrammes}}{\text{quadratmillimeter.}}$
" copper, 1950 "
" brass, 1600 "

The velocity of sound was measured in the same species of wires by stretching them in a Weber's monochord, and by varying the length, comparing the longitudinal tone with tuning forks. The normal tone of the set of tuning forks was determined by comparison with two normal tuning forks of Appunn.

The modulus of dilatation of the wire (i. e. the length of a wire of the same species possessing the weight necessary to double the original length), is obtained from the velocity of sound c by means of the formula $A=\frac{c^2}{g}$, where g denotes the acceleration due to gravity. The value A′ ordinarily employed in practice, and made use of by Wertheim, (i. e., the weight which hung on a wire whose section is unity would double the length), is obtained as above by multiplication with the density. Thus it was found

	Velocity of sound.	Modulus of elasticity. A.	A′.
Iron,	5050 meters.	2580 kilometers.	20310 $\frac{\text{kgr.}}{\text{quad. mill.}}$
Copper,	3640 "	1350 "	12140 "
Brass,	3380 "	1170 "	9810 "

Wertheim gives A′ for iron wire =19445, copper =12536, brass =9000 $\frac{\text{kgrs.}}{\square\,\text{m.m.}}$

According to these observations, the moduli of torsion bear to the moduli of dilatation, for all three wires very nearly the same ratio. The ratio $\frac{A}{T}$ is

for iron, 5·85
" copper, 6·23
" brass, 6·15
Mean, 6·07

The deviations of these values from the mean are perhaps due to errors of observation, especially in the determination of the densities, since in order to be sure of investigating the same material as in the original experiments, but small masses could be employed. It is possible too that the deviation of the mean from the whole number 6, is to be sought in these errors.

Without entering on a more extended investigation of this point, and without discussion as to whether the customary theory of elasticity is applicable to such thin wires, or whether the lack of isotropy, or the relations of superficies, forbid this application, it may be remarked that according to the theory

$$\frac{A}{T} = 4(1+\mu),$$

where μ denotes the ratio of the transversal contraction to the longitudinal dilatation. By a comparison of the torsional and longitudinal vibrations, there would result accordingly for our three wires very nearly the same value for μ. If it is assumed that $4(1+\mu)=6$ there results $\mu=\frac{1}{2}$. This is the extreme value permitted by the theory, which would correspond to a volume unchanged by dilatation.

It may suffice to have indicated the fact. It would lead us too far from the chief object of our investigation to develope the subject more in detail.

ART. XXXVIII.—*On the Oxy-Calcium Light as applied to Photo-Micrography;* by Lieut. Col. J. J. WOODWARD, Assistant Surgeon, U. S. Army. Report to the Surgeon General of the U. S. Army, dated June 4, 1870.

SINCE the preparation of my report of January 4, 1870, on the use of the Magnesium and Electric lights in Photo-micrography, I have made some experiments with the Oxy-calcium, or Hare's light, as a source of illumination for the same purpose, and have succeeded in obtaining excellent pictures with powers as high as a thousand diameters. This result appears to me of considerable importance, both because of the comparative cheapness of this light, and because the apparatus for its production is so common as to be practically within the reach of every microscopist. In addition to these advantages the oxy-calcium light possesses the quality of steadiness to a greater degree than either the electric or the magesium lamp and requires much less trouble and skill to manage.

For the purposes of my experiments, I made the hydrogen as I consumed it, in a Hare's self-regulating generator, by the action of dilute sulphuric acid on scraps of ordinary sheet-zinc.

The oxygen was sometimes made in the usual way from chlorate of potassa, sometimes purchased compressed in iron cylinders; in either case it was transferred to a large sheet-iron gasometer for use. The gases were burned under a pressure equal to a column of water fourteen inches high. I used for lamp one of the first-class magic-lanterns, manufactured by J. W. Queen & Co. (No. 924, Chestnut street, Philadelphia, Pennsylvania,) in which the disc of lime is revolved by clock-work before the burning jet of gas and a fresh surface constantly presented to the flame. I simply removed from the lantern the lens intended to magnify the image on the slides, when the apparatus is in ordinary use, and allowed the cone of light proceeding from the large condenser of the instrument to fall upon the achromatic condenser of the microscope, in the same manner as described and figured for the magnesium lamp in my report of January 4th, a reference to which will render any description of the arrangement of the microscope and of the sensitive plate unnecessary in this place.

I employed the ammonio-sulphate cell, as I do in taking Photo-micrographs with other sources of light, but found I could dispense with the ground glass which is necessary in photographing so many objects, if sunlight or the electric lamp is employed; a large portion of the lime disc being luminous, the resulting mixed divergent pencil, like that obtained from the magnesium lamp, does not produce the interference phenomena which result when tissues and many other objects are illuminated by powerful parallel rays. This circumstance, however, renders the calcium light inferior to the sun and the electric lamp, in the resolution of the Nobert's plate and certain lined test objects.

I did not find the time of exposure differed materially from what I had given in making photographs of the same objects with the magnesium lamp, and the pictures produced were not inferior to these in quality. This arose from the fact that the greater steadiness of the calcium light permitted the use of condensers which concentrated the light to a greater degree than I had found advantageous with the magnesium lamp, and not from equality in the actinic power of the two sources of illumination. I have recently made some experiments with the view of obtaining positive information with regard to the comparative actinic energy of the electric, magnesium and calcium lamps which I employ. For this purpose all condensers being removed the divergent pencil proceeding from each lamp in turn was permitted to fall, for the space of five seconds, on an exposed circular portion of a sensitive plate thirty feet distant.

The whole operation was completed in less than a minute, when the plate being developed in the ordinary way three cir-

cular spots appeared as the results of the exposures. The spot produced by the electric light was intensely black, that by the magnesium of a rich middle-tint, while the circle impressed by the calcium light was extremely pale. Want of time prevented me from continuing these experiments and obtaining as I desired numerical values for the relative actinic powers of these sources of illumination under definite conditions; this I have however regretted the less, as the actual energy of the naked flames is not really the measure of their availability in photo-micrography; here the question of steadiness, involving, as it does, the possibility of great concentration, plays a most important part and materially modifies the result.

So far as I know, the Calcium light has never before been successfully employed as the source of illumination for making Photo-micrographs in this country. My friend Dr. R. L. Maddox, however, writes me that it has been experimented with in England by Drs. Abercrombie and Wilson. He thinks they used powers as high as an eighth with pleasing results. This information has directed my attention to the essay of Dr. Wilson in the Popular Science Review for 1867, volume vi, page 54, in which that gentleman gives in detail the process employed by himself and Dr. Abercrombie. He experimented with an oil lamp and with the Oxy-calcium and magnesium lights: "I can scarcely think it would be used now that the more active light of magnesium is within the reach of every one." And of the magnesium: "The light fails only in steadiness, and if some means could be devised for burning the metal uniformly and at a fixed point nothing would be left to desire." Dr. Beale (How to work with the Microscope, 4th edition, p. 248) tells us that some of the pictures of these gentlemen were remarkably good, "they possess a peculiar delicacy in the half tones and the shadows, with much roundness of the objects, but the definition, as might be expected, does not quite equal in some of the finest markings, prints obtained from sun negatives." A perusal of Dr. Wilson's paper will show that my process differs from his in the use of the following precautions: the interpolation of an ammonio-sulphate cell to exclude the non-actinic rays, the use of lenses specially made for photography for all powers from the $\frac{1}{8}$th down, the use of much larger condensers to concentrate the light and so to shorten the exposure, and in the case of the magnesium light, in the use of a clock-work lamp to increase the steadiness of the illumination. Each of these points are in my judgment essential to obtain the best results.

I learn from the same letter of Dr. Maddox that he had himself made experiments with the magnesium lamp some time before those of Abercrombie and Wilson. He used powers as

high as a fifth, and appears to have obtained better results than I supposed any one had done prior to the publication of my report. He gives me the following account of his experience: "The first picture I took with the magnesium light was done in a very rude way. An inch and a quarter of wire was held in a small spirit flame and advanced by hand as burnt. The objective was Beck's $\frac{2}{3}$ds, the object the sycamore-leaf insect, and about $\frac{2}{3}$ths of an inch of wire remained after use. I sent a print, with a sun print of the same to the British Journal of Photography, and in the number for July 1, 1864, you will find some remarks by myself and the editors. Now to try and meet any error that might arise from what we may term want of correction, I used the $\frac{2}{3}$ds with the correcting lens, which is excellent for sunlight; the picture was soft, full of half tone, but wanted, as in other pictures I had seen by artificial light, the decision of definition in the outlines." "After this I used the $\frac{1}{5}$th with the little apparatus sketched in Beale's book (page 275) and which I venture to think, embraces all that is required for its use, provided the condenser has its focus at the burning point, and that the reflector has the same." With the $\frac{1}{5}$th, fibers of cotton and muscular fibrillæ of boiled shrimp, with several other objects were taken, but I did not use any higher power, nor indeed pay much attention to the subject as I gave the preference to the sunlighted prints and negatives." I give these extracts with great pleasure as showing the experience in this direction of one of the most distinguished laborers in the field of Photo-micrography, and regret that I was not acquainted with them at the time of publishing my first report. The method of Dr. Maddox, however, differed from mine in the same essential points as that of Abercrombie and Wilson, and the peculiar fitness of the magnesium light for photographing the animal tissues and those objects generally, which require the use of ground glass when sunlight is employed, would appear to have escaped the observation of these accomplished gentlemen, and to have remained unnoted until the publication of my report.

In conclusion I append to this paper two illustrative photographs. The first, which represents the 6th square of the Möllers type-plate of the diatomaceæ, taken with Wales's $1\frac{1}{2}$ inch objective, arranged to give thirty-five diameters, will serve for comparison with the photographs of the same object with the same lens taken by sunlight and by the electric and magnesium lamps, which were published with my former report. The second represents the Navicular Lyra, taken with the Powell and Lealand's immersion $\frac{1}{16}$ arranged to magnify 1000 diameters.

Art. XXXIX.—*On the Secular Perturbations of the Planets;* by Asaph Hall.

In view of the recent consideration in geological speculations of the secular inequalities of the excentricity of the earth's orbit, it may be worth while to state briefly the method of treating secular perturbations, and our present knowledge of the subject.

Denoting by e, π, φ, and θ the excentricity, the longitude of the perihelion, the inclination of the orbit and the longitude of its ascending node, it has been found easier in the discussion of the problem to substitute for these quantities four others which are simple functions of them; and thus are assumed

$$h = e \sin \pi. \qquad p = \text{tang } \varphi \sin \theta.$$
$$l = e \cos \pi. \qquad q = \text{tang } \varphi \cos \theta.$$

Neglecting terms of the third and higher orders of the excentricities and inclinations in that part of the development of the perturbative function from which the secular perturbations ari e, linear differential equations of the first order are established containing the first differential co-efficients of h, l, p and q with respect to the time; and it is by the integration of these equations that the secular perturbations are obtained. By this process also the equations are separated into two distinct classes admitting of separate treatment; the one class containing the differential co-efficients of h and l and the solution furnishing the values of the excentricities and the longitudes of the perihelia, and the other containing those of p and q and the solution giving the values of the inclinations and the longitudes of the ascending nodes. In this way the discussion of the problem is much simplified. The numerical co-efficients in the differential equations depend on the semi-major axes of the orbits, and on the masses of the planets. Considering the eight principal planets of our solar system, there will be of course eight values of h, l, p, and q, and these are usually distinguished by the addition of accents to the symbols.

In order to effect the integration, Lagrange, whose method is still followed, assumed

$$h = \text{N} \sin (g\,t + \beta), \qquad l = \text{N} \cos (g\,t + \beta),$$
$$h' = \text{N}' \sin (g\,t + \beta) \qquad l' = \text{N}' \cos (g\,t + \beta), \qquad \&c.$$

Differentiating these equations and substituting the value of $\frac{dh}{dt}, \frac{dh'}{dt}, \frac{dl}{dt}$, &c., in the differential equations and then eliminating the ratios of the coefficients N, N′, N″, &c., a numerical equation is obtained for the determination of g. This equation will

rise to the degree denoted by the number of planets considered, and in our solar system will be of the eighth degree. If we denote by $g_1, g_2, g_3 \cdots g_8$ the roots of this equation the general integrals will be

$$\begin{aligned}
h &= N_1 \sin(g_1 t+\beta_1)+N_2 \sin(g_2 t+\beta_2)+\cdots+N_8 \sin(g_8 t+\beta_8),\\
l &= N_1 \cos(g_1 t+\beta_1)+N_2 \cos(g_2 t+\beta_2)+\cdots+N_8 \cos(g_8 t+\beta_8),\\
h' &= N'_1 \sin(g_1 t+\beta_1)+N'_2 \sin(g_2 t+\beta_2)+\cdots+N'_8 \sin(g_8 t+\beta_8),\\
l' &= N'_1 \cos(g_1 t+\beta_1)+N'_2 \cos(g_2 t+\beta_2)+\cdots+N'_8 \cos(g_8 t+\beta_8),\\
&\quad \&c., \qquad\qquad \&c.
\end{aligned} \tag{1}$$

The arbitrary quantities $N_1, N'_1 \cdots \beta_1, \beta_2$, &c., are determined by the initial values of h and l. The solution for p and q is quite similar to that for h and l.

The conditions necessary for the stability of the system are, first, that the eight roots of the equation for g shall be real and unequal, in order that outside the circular functions there may be no terms containing the time as a factor or exponent and which would therefore increase indefinitely; and secondly, it is necessary that the coefficients N may not be great in order that the excentricity may not increase so as to render divergent the series which have been assumed in the solution to be rapidly convergent.

The actual numerical solution by several eminent astronomers, Lagrange, Pontecoulant and Leverrier, their results being essentially accordant in this respect, shows with a great degree of probability that our solar system is a stable one, the law of gravitation alone being considered; although to speak with certainty on this point an analytical solution is to be desired. But when it is required to compute for very remote epochs the values of the elements of the orbits the co-efficients g which in equations (1) are multiplied by the time must be carefully considered. These coefficients depend on the assumed masses of the planets, and are generally determined by neglecting terms of the third order. The most complete investigation of this subject is that given by Leverrier in the Connaissance des Tems for 1843 and 1844, and reproduced with some additions in the memoirs of the Paris Observatory for 1856. In this work, which is a masterpiece of astronomical calculation, Leverrier shows that terms of the third order may produce corrections of the values of g amounting to three or four tenths of a second. Probable uncertainties in the assumed values of the masses of the planets may give rise to errors of nearly two tenths of a second. Hence, if we consider the forms of the general integrals (1), we shall readily see that for very remote epochs, distant by millions of years, our calculations must be very untrustworthy; since when the time is great the errors in the values of g may completely change the character of the circular functions.

For a satisfactory solution of this problem, certainly one of the most interesting in astronomy, our knowledge of the masses of the planets must be greatly advanced. In the case of the interior planets it appears that we must wait patiently until the theory of their own motions, or the motion of some one of the periodical comets, shall furnish the data for an exact determination of their masses. The masses of Mars and Jupiter will in time be very accurately known from the theories of some of the minor planets. But in the case of Saturn, Uranus and Neptune it appears to me that the instrumental means are already at hand for making an accurate determination of their masses, and a more complete investigation of the theories of their satellites. When the novel and entertaining observations with the spectroscope have received their natural abatement and been assigned their proper place, it is to be hoped that some of the powerful telescopes recently constructed may be devoted to this class of observations, where a rich and an ample field awaits the skillful observer. One could not wish a better example than the beautiful work of Bessel on the satellites of Jupiter.

August 2, 1870.

ART. XL.—*Farmer's Theorem discussed;* by FRED. E. STIMPSON.

TOWARD the close of Prof. Silliman's paper, "On the relation between the intensity of light produced from the combustion of illuminating gas and the volume of gas consumed" (this Jour., xlix, 17), is the following:—"A comparison of the foregoing results will show that the coincidences, with the requirements of the theorem of Farmer are, within the limits assigned, too numerous, and too closely accordant, to be considered as otherwise than pointing clearly to its general truth."

What I propose to examine now is, whether the data given in the paper referred to, do warrant the conclusion reached.

According to the data given for the first experiment, two lights were made equal, so that the disk of a Bunsen Photometer stood midway between the flames, and the consumptions were found to be 3·66 feet per hour.

"The screen was then moved upon the bar to a point just four times as far from one flame as it was from the other, i. e., the bar being 100 inches, the screen stood at 80, i. e., as 1 : 4. The light from the distant burner was then increased, until the disk again showed as an equality of illumination. On reading

the rate of the gas consumed by the two burners respectively, one gave 3·66 cubic feet and the other 7·32 cubic feet."

Now from this it will be seen that the ratio of consumption was as 1 to 2, but the ratio of the lights being as the squares of the distances from the disk, becomes $200^2 : 80^2$ or 1 : 16, and the result on the paper should stand,

$$3{\cdot}66^4 : 7{\cdot}32^4 = 1 : 16 \text{ and not}$$
$$3{\cdot}66^2 : 7{\cdot}32^2 = 1 : 4.$$

In other words, the lights were proportional to the fourth powers of the consumptions and not to the squares.*

In experiments 2, 3 and 4, the ratio of the squares of consumption is nearer to the ratio of the lights than the simple ratio of the consumptions, but in experiment 2, the ratio of the 2·67 powers of the consumption almost exactly expresses the ratio of the lights. In experiment 3 the ratio of the squares is too small, and in experiment 4 it is too great; while in experiment 5, the simple ratio is certainly much nearer than the ratio of the squares.

The following table will, I think, suffice to show these various relations:

No. Exp't.	Rate of consumption per hour.	Intensity found by experiment.	Intensity by old law.	Intensity by law of the squares.	Intensity by the 2·67 power of consumption.	Difference between calculated and observed results.		
						Old law.	Law of squares.	2·67 power.
2	3·30	1·	1·	1·	1·	0	0	0
	4·35	2·1	1·32	1·73	2·098	−0·78	−0·37	−0·002
	5·136	3·2	1·55	2·42	3·222	−1·65	−0.78	+0·022
	5·556	4·0	1·68	2·83	3·995	−2·32	−1·17	−0·005
3	3·72	1·	1·	1·	----	0	0	----
	4·884	2·	1·312	1·72	----	−0·628	−0·28	----
	6·000	3·	1·61	2·60	----	−1·39	−0·40	----
	7·218	4·	1·94	3·76	----	−2·06	−0·24	----
4	4·5	1·	1·	1·	----	0	0	----
	9·519	4·	2·114	4·46	----	−1·886	+0·46	----
5	5·16	1·85	1·85	1·85	----	0	0	----
	10 06	4·00	3·70	5·72	----	−0·30	+1·72	----

In experiment 6, the reductions have been made by applying the old rule to the correction for the candle in both cases; the correction by Farmer's Theorem being applied only to the consumption of the gas, and so applied gives the most concordant results, the difference shown by the old rule however, (1·34 candles) is no greater than might have occurred between two observations, even if the consumption in both cases had been

[* Since the above was written, I have been informed that the error is not in the proportion of ("1 to 4") but in the statement of the position of the disk upon the bar. It should read *twice* as far from one flame as the other, i. e., the screen stood at 66⅔, and thus the experiment supports the theorem.]

equal, i. e., five feet per hour. But this experiment alone certainly does not warrant the conclusion reached, viz:—"That this theorem applies with equal force to the weight of sperm consumed by the standard candle as to the volume of the gas burned in equal times," because the correction has *not* been applied to the candle either here or in any other observation given in Prof. Silliman's paper.

The confirmation obtained by experimenting upon Peytona, Albert and Wollongong gas depends upon the assumption that the true candle power of a rich gas can be obtained by mixing it in definite proportions, with another gas whose illuminating power is known, and deducing from the observed candle power of the mixture, the candle power of the rich gas; until this assumption has been proved to be correct, it is of course useless in establishing Farmer's Theorem.

The next proof offered is drawn from a tabular statement in Sugg's Gas Manipulation. Prof. Silliman says, "By this statement the burner in question produced from five cubic feet of gas exactly 15 (14?) candle power, but when reduced to 4·5 cubic feet consumption the candle when 'corrected to the standard quality of gas by proportion' was only 11·93 candles. The values of the 'correction' referred to can only be conjectured, but assuming that the observation made the uncorrected rendering 11·32 candles (a very probable quantity), we find that the law of the squares of consumption then makes the ratio as follows:—$4{\cdot}5^2 : 11{\cdot}32 = 5^2 \cdot 14$." The assumption here made is not at all necessary because we can find the exact value of the 'correction' for the gas by reversing the proportion used, thus:—$5 : 11{\cdot}93 = 4{\cdot}5 : 10{\cdot}73$, and now applying Farmer's Theorem the ratio becomes $4{\cdot}5^2 : 10{\cdot}73 = 5^2 : 13{\cdot}24$ only. The relation can be exactly expressed by the ratio $4{\cdot}5^{\frac{5}{2}} : 10{\cdot}73 = 5^{\frac{5}{2}} : 13{\cdot}997 = 14$.

The last proof offered is drawn from Audoin and Berard's experiments. But after several fruitless efforts to obtain the results, as given under the head of "Intensities by law of the squares of consumption," I am forced to conclude that these figures are incorrect through some inaccuracy in applying that law. The two tables referred to may be found in the author's original memoir on pages 439 and 441 of Annales de Physique et de Chimie, vol. lxv, 1862. In these experiments they compared the burning of two batwing burners at different rates of consumption, with a "Bengel Argand" whose rate of consumption was nearly constant. The first three columns of the table below give their experimental data.

TABLE I. *Burner of the fifth series—slit $\frac{1}{8}\frac{1}{5}$ inch wide.*

Consumption of the Batwing under trial. Liters per hour.	Consumption of the Bengel Argand. Liters per hour.	Comparative intensities. The Bengel burner being 100	Intensities by old formula or direct ratio of consumption. l'	Difference between calculated and observed results.	Intensities for law of the squares of consumption. l''	Difference between calculated and observed results.
211	100	120	99·38	−0·62	82·31	− 17·69
189	96	110	97·64	−2·36	86·38	− 13·62
180	103	100	100·00	0·00	100·00	0·0
156	104	90	104·85	+4·85	122·17	+ 22·17
142	104	80	102·39	+2·39	131·06	+ 31·06
124	101	70	99·65	−0·35	141·84	+ 41·84
102	101	60	103·82	+3·82	179·68	+ 79·68
88	102	50	99·30	−0·70	197·19	+ 97·19
68	100	40	102·79	+2·79	264·20	+164·20
57	102	30	90·73	−9·27	293·40	+193·40

TABLE II. *Burner of the same series—slit $\frac{1}{6}\frac{1}{3}$ inch wide.*

Consumption of the Batwing under trial. Liters per hour.	Consumption of the Bengel Argand. Liters per hour.	Comparative intensities. The Bengel burner being 100.	Intensities by old formula or direct ratio of consumption.	Difference between calculated and observed results.	Intensities for law of the squares of consumption.	Difference between calculated and observed results.
264	106	200	100·4	+ 0·4	50·3	− 49·7
236	105	180	100·1	+ 0·1	55·6	− 44·4
208	105	160	100·9	+ 0·9	63·5	− 36·5
182	105	140	100·9	+ 0·9	72·8	− 27·2
152	104	120	102·6	+ 2·6	87·7	− 12·3
130	104	100	100·0	0·0	100·0	0 0
112	104	80	92·8	− 7·2	107·8	+ 7·8
90	104	60	86·6	−13·4	125·1	+ 25·1
75	104	50	86·6	−13·4	150·2	+ 50·2
66	104	40	78·8	−22·2	155·2	+ 55·2
43	104	20	60·5	−39·5	182·8	+ 82·8
28	104	10	46·4	−53·6	215·5	+115·5

From the first table it will be seen that the two burners gave equal light (intensity 100), when the batwing consumed 180 liters per hour, and the standard 103 liters. Correcting the first line of this table for these consumptions, by the old rule we have for the batwing $211 : 180 = 120 : l$
and for the standard $103 : 100 = l : l'$;
the total correction for both will be therefore $211 \times 103 : 180 \times 100 = 120 \times l : l \times l'$;
the l cancels out and leaves the proportion $211 \times 103 : 180 \times 100 = 120 : l'$.
The second line of this table becomes $189 \times 103 : 180 \times 96 = 110 : l$
and so on.

Now applying Farmer's Theorem, the proportions become

$$(211)^2 \times (103)^2 : (180)^2 \times (100)^2 = 120 : l''$$
$$(189)^2 \times (103)^2 : (180)^2 \times (96)^2 = 110 : l''.$$

The value of l' and l'' from these proportions will be found in the 4th and 6th columns of the above table.

It will be seen that in the second table the burners gave equal light for consumptions of 130 and 104; these numbers have been used therefore in the corrections for that table.

The experiments show that the last term of the proportions should be 100. The tables show that by the old formula, this term becomes about 100 for every experiment of the first table, and for the first seven experiments of the second table, the greatest difference being 7·2; and for the remaining five experiments, the old formula gives the best approximate results except for the last one, and here the old law gives a result which must be *multiplied* by 2·15 to make it correct, and Farmer's Theorem gives a result that must be *divided* by 2·15 to make it correct.

From a perusal of these various results, I am led to disagree with Prof. Silliman, and say that 'Farmer's Theorem' is not proven, and that the law of the squares does *not in general* give any closer results than the old law of the direct ratio; though I entirely and heartily concur with him in the conclusion, *that every photometric observer should recognize the importance of bringing the consumption of gas and sperm to the agreed standard*, when attempting to give the true candle power of any gas.

It is much to be desired that experimenters should turn their attention to the matter of the relation of consumption of gas to illuminating power, and I sincerely hope that Mr. Farmer will not let the matter rest here, but will make and publish further observations upon the same subject.

Messrs. Audoin and Berard in their valuable experiments, to determine the best burner for the city of Paris, proved that for every consumption of gas there is a burner which is best suited for that consumption. Now by the proper selection of a burner for small consumptions, some simple relation may yet be found between consumption and illuminating power.

In the above, reference has been made only to experiments detailed in Prof. Silliman's communication. I have, however, made a number of experiments myself upon the same subject, besides collating the results of some sixty or more independent observations, which have been published during the last fifteen years, and the results are curious, instructive and unexpected. There are a few among them to which Farmer's Theorem might be applied, quite a number to which the old law will apply; though many of them require a *modification* of the old law.

I hope in due time to prepare a paper giving some of these results.

April, 1870.

ART. XLI.—*Note on Mr. Stimpson's Paper on Farmer's Theorem;* by B. SILLIMAN.

MR. STIMPSON'S criticism on the first experiment of my paper was induced by an obvious numerical error in the statement of the data, which Mr. S. has himself corrected in a foot note on p. 373. The experiment properly stated exactly sustains the theorem.

I admit that in Experiment No. 2, the exponent comes nearer to the third power than to the square, being 2·689, 2·638 and 2·669; in the three cases average 2·661. But it also indicates that the illuminating power of the standard 3·30 cubic feet was very imperfect. The experiments, however, appear to me to demonstrate clearly the radical inaccuracy of the old rule for photometric calculations, and that some ratio near to or greater than the square gives often more trustworthy results than the old rule.

In Experiment No. 3, the exponents are respectively 2·552, 2·297 and 2·092, coming in the last very near the square. It shows that 4·88 cubic feet consumption gave the best results, and also that 3·72 and 7·219 cubic feet consumption gave very nearly the same degree of intensity of combustion.

In Experiment No. 4, the exponent 1·85 power for a consumption of 9·519 consumption, shows an imperfect combustion of the fish-tail burner employed.

In Experiment No. 5, the experimental conditions were wholly unfavorable to accuracy, owing to an inequality of pressure unavoidable in the experimental method adopted, there being one inch pressure on the 10·06 c. f. consumption, and only half an inch on the 5·16 c. f. It is well known to all photometric observers how important a low pressure and an equal pressure is to the results obtained. It was hardly fair to Mr. Farmer to have quoted this trial, but I was desirous of exhibiting the entire range of observation, bad as well as good.

As in Experiment No. 6, the consumption of sperm was in the two tests very nearly uniform; the difference would be very trifling if the correction had been applied. Nor is it by any means so certain that Farmer's Theorem applies to the candle as I supposed when the remarks quoted by Mr. Stimpson were made, since, if a candle burns much over 120 grains it smokes ("tails off"), and then there is an end of all accuracy, and the observation must be rejected; since there is an imperfect combustion giving an increase in consumption but not in intensity.

Mr. Stimpson rejects all the data given by me which are founded upon the determination of the intensity of a rich gas

by the 'method of mixtures,' because, as he says, the accuracy of this method has not yet been demonstrated by experiment. But this objection ceases to have force now that experiments, made by Mr. Farmer and myself lately, prove that this method is worthy of confidence. Inasmuch as the most important case for the use of Farmer's Theorem is that of gas too rich to be burned in the standard burner on a basis of 5 cubic feet per hour, it is striking off by all means the most valuable portion of my contribution to photometrical methods if it could be shown that the 'method of mixtures' was untrustworthy. I am, therefore, glad of this occasion to reiterate my confidence in this method, and to refer the reader curious in such matters to a brief paper of mine upon this subject, which will be found on page 379.

I care very little whether the results of experiment shall show, when they are sufficiently accumulated, that the ratio of consumption of gas is to the intensity produced as the squares of consumption in a given case, or in some other ratio greater than a simple ratio. I have desired chiefly to call attention to the general untrustworthiness of all photometric observations which are made with volumes of gas much less than the normal standard adopted, when these results are calculated on the simple ratio of the consumptions. It is only by the accumulation of carefully conducted experimental data that a law can be established, and these data are now pretty rapidly accumulating.*

* In the proceedings of the American Association for Advancement of Science, Salem meeting, I presented the matter referring to *Sugg's manipulation* in a different form, giving the results of the observations with his Argand burner in a tabular form as follows:

No. obs.	Cubic feet.	Observations corrected by old rule candles.	Differences.	Uncorrected observations.	Observations corrected by Farmer's Theorem.	Differences.
1.	5·	14·00		14·00	14·00	
2.	4·9	13·78	0·22	13·504	14·060	0·556
3.	4 8	13·74	0·26	13·190	14·312	1·132
4.	4·7	13·30	0·70	12·502	14·148	1·646
5.	4·6	13·04	0·96	11·996	14·191	2·105
6.	4·5	11·93	2·07	10·738	13·255	2·517

The mean candle power of the 6th column is 13·99 candles; difference 0·01 candle.

The following will show the fractional power required to bring the uncorrected observations (column 5) to 14·00 candles.

2.	$4{\cdot}91^{\cdot83}$: 13·504	: :	$5^{1\cdot83}$: 14·00	candles.
3.	$4{\cdot}81^{\cdot47}$: 13· 90	: :	$5^{1\cdot47}$: 14·00	"
4.	$4{\cdot}71^{\cdot85}$: 12·502	: :	$5^{1\cdot85}$: 14·00	"
5.	$4{\cdot}61^{\cdot86}$: 11·996	: :	$5^{1\cdot86}$: 14·00	"
6.	$4{\cdot}52^{\cdot52}$: 10·737	: :	$5^{2\cdot52}$: 14·00	"

This table shows that the 3rd and 6th tests have not been good ones, while the 2d, 3d, 4th and 5th tests fall a little below the square or 2d power, and the 6th test is considerably more.

New Haven, July, 1870.

ART. XLII.—*On the Determination of the Photometric Power of a rich gas by dilution with a poor gas of known value: the "method of mixtures;"* by B. SILLIMAN.

IN a paper on 'Farmer's Theorem,'* I have given several examples of the method of determining the intensity or photometric power of a rich gas by diluting it with several times its own volume of a poorer gas of known intensity, and then calculating its value from the increment of intensity. Having demonstrated in the paper before mentioned the worthlessness of all determinations of the intensity of gases of high illuminating power made by burning them in volumes less than five cubic feet, and then calculating their intensity by the rule of three up to that volume, I have shown how much more exact results were obtained when the results were calculated upon the theorem of Mr. Farmer; this greater exactness being predicated largely upon the confirmation drawn from parallel observations upon the same gases when measured by the method of mixtures. The results thus obtained having, however, been questioned by Mr. Stimpson,† on the ground that the method itself had not been experimentally demonstrated, I have undertaken lately, in connection with Mr. Farmer, to make some experiments calculated to test its accuracy.

The results which go to support the accuracy of the method were obtained with the use of a new photometric apparatus, constructed for the Manhattan Gas Co., under my direction, by Sugg of London, and which was designed to embrace all the best approved features which recent experience has indicated in photometry. A discussion of these details would be out of place in this connection. Before detailing our results, it will be proper to present the method of determination of intensity for gas of high illuminating power as practiced by Mr. Farmer at the Manhattan Gas Works in New York, and which I have called *the method of mixtures.*

To find the candle power of a gas having, for example, an intensity greater than 20 candles, mix the rich gas of unknown power with a poorer gas of known power in such proportions that the intensity of the mixture shall not be greater than 20 candles power, when consumed at the agreed rate of not over five cubic feet per hour. Then to compute the candle power (intensity) of the rich gas,—

* This Journal, II, xlix, 17; also Proceedings of American Association for Advancement of Science, Salem meeting, 1869, p. 149.

† See page 272, this volume.

Let a=the percentage or volume of gas of low intensity.
" b=the intensity in candles of the gas of low intensity.
" c=the percentage or volume of rich gas used in the mixture.
" d=the intensity in candles of the mixture as observed.
" x=the intensity in candles of the rich gas required.

Hence, $\frac{ab+cx}{a+c}=d$ $\qquad$ $\frac{(d-b)a}{c}+d=x$

$$ab+cx=ad+cd$$
$$cx=ad+cd+ab.$$

And this expression is stated arithmetically in the following

Rule:—Subtract the intensity of the poor gas from the intensity of the mixture; multiply the remainder by the volume of poor gas; divide the product by the volume of rich gas; add to the quotient the intensity of the mixed gas, and the sum is the intensity of the rich gas sought.

Now when we reflect that in any given illuminating gas we have always a certain volume of non-luminous combustible gas, as the substratum to which is added, according to its source and mode of treatment, a variable volume of illuminants, it is no unwarranted assumption to say that the illuminants (chiefly olefines) are *diluted* by the non-illuminants. It is agreed, on all hands, that hydrogen, marsh gas and carbonic oxyd, which together form the mass of the non-luminous substratum of all illuminating gas, have of themselves, when pure, no luminosity, and when burned at ordinary atmospheric pressures and corresponding temperatures, that they may in fact be called, as to luminosity, neutral. It can hardly be questioned that the intensity which these neutral gases may assume in a given mixture must depend, under the same ordinary conditions before mentioned, upon the amount and kind of olefines they may take up in the destructive distillation of the coal or other hydrocarbons used in making gas. If these assumptions are true, we ought to be able to demonstrate them by experiment, by commingling certain volumes of a gas of known intensity with a neutral gas of no intensity. These experimental conditions would be met by using carbonic oxyd as the neutral gas, or better still, the mixture of carbonic oxyd and hydrogen resulting from the decomposition of vapor of water at a high temperature in contact with highly ignited carbon, as in the hydrocarbon gas process. But in default of any convenient means of obtaining these gases, we had recourse to hydrogen gas evolved by the action of diluted sulphuric acid upon zinc in a large self-regulating generator of hydrogen. It is well known that hydrogen thus made is not absolutely non-luminous, but it is sufficiently so for photometric purposes. The avidity of hydrogen for all the olefines, however, renders it difficult to

obtain entirely satisfactory results with the use of this gas, provided it is passed through gas pipes and holders which have been previously used for the transmission of coal gas, since however carefully one may rinse out these tubes by hydrogen, there may yet cling, probably, some small trace of the condensed illuminants to the walls of the tubes, which imparts a trifling intensity to the hydrogen passing through them.

To obtain a supply of rich gas of uniform intensity, 100 lbs. of Albertite were coked until 810 cubic feet of gas had been taken from it of a density of ·498. This gas was purified, collected and preserved in a separate gas holder. Its intensity was determined,

1st.	By Farmer's Theorem	= 30·49	candles.
2d.	" mixing with poor gas	= 30·95	"
3d.	" simple ratio	= 26·20	"

In determining the intensity of this gas by the method of mixtures, 20 per cent by volume of Albert gas was mingled with 80 per cent of 10·6 candle gas obtained from a poor coal. The mixture had an intensity of 14·67 candles. Hence,

$$(14{\cdot}67-10{\cdot}60)\times 80\div 20+14{\cdot}67=30{\cdot}95 \text{ candles}$$

for the intensity of the Albert Gas by the method of mixtures.

1st Experiment.

Taken	75 volumes of	coal gas	=14·75	candles, obs'd.
"	25 "	hydrogen	= ----	
"	100 "	mixture	=10·22	" "

If hydrogen=0 this mixture should have given 11·06 " calc.
Hence there is an error of observation of ·84 of a candle, which makes the value of the hydrogen apparently below zero.

2d Experiment.

Taken	75 volumes of	coal gas	=14·25	candles, obs'd.
"	25 "	hydrogen	= ----	
"	100 "	mixture	=10·35	" "

If hydrogen=0 this mixture should have given 10·69 " calc.
Hence there is an error of observation = ·25 " which again makes the value of the hydrogen, apparently a little below zero.

The want of sufficient storage room for hydrogen and the necessity of many repetitions, and much care in manipulation to avoid errors of quantity in the synthesis of mixtures make experiments of this sort tedious, and my other avocations have prevented my multiplying them as much as is desirable. I think, however, that most photometric observers will agree with me that it is safe to conclude, from these two experiments, that the action of hydrogen in gaseous mixtures is simply that of a

diluant. We might make a thousand observations by the means now at command, and not obtain one with an exact 0 for hydrogen. A very trifling error in the admeasurement greatly influences the result.

We may therefore safely conclude, as it appears to me—

1st. That in all illuminating gas we have a certain substratum of non-luminous gas, holding in solution a variable volume of luminous gas (olefines).

2d. That when a gas is too rich in illuminants to permit of accurate photometric admeasurement by the usual standards of intensity, it may be diluted with a poor gas of known value and volume to such a standard as is consistent with the accurate employment of the usual photometric apparatus, its true value being then calculated from the known values employed.

P. S. I find in the manuscript records of the Manhattan Gas Company, mention of four experiments made many years since by Mr. Schultz, chemist of that company, in which he mixed 5 per cent and 10 per cent of hydrogen with gas of very high illuminating power. The results are less satisfactory than they would have been, had the volume of hydrogen employed been much larger and the intensity of the coal gas not over 15 candles. Moreover at that time the means of admeasurement at the command of the observer in the laboratory of the Manhattan Company of small volumes of gas were much less exact than they now are. The results obtained are as follows:

1st Experiment.

Taken	90 volumes of coal gas		=22·53	candles,	observed.
"	10 "	hydrogen	=----		
"	100 "	mixture	=19·11	"	"
If the hydrogen is taken as 0		"	=20·27	"	calculated.
Showing apparent error of			=— 1·11	"	"

2d Experiment.

Taken	95 volumes of coal gas		=22·53	candles,	observed.
"	5 "	hydrogen	= ----		
"	100 "	mixture	=20·35	"	"
If the hydrogen is taken as 0		"	=21·39	"	calculated.
Apparent error			=—1·04	"	"

3d Experiment.

Taken	90 volumes coal gas		=20·83	candles,	observed.
"	10 "	hydrogen	= ----		
"	100 "	mixture	=19·44	"	"
If hydrogen is taken= 0		"	=18·29	"	calculated.
Apparent error,			+ ·15	"	"

4th Experiment.

Taken	95	volumes of	coal gas	=20·83	candles,	observed.
"	5	"	hydrogen	= ----		
"	100	"	mixture	=20·52	"	"
If hydrogen is taken	=0		"	=19·79	"	calculated.
Apparent error,				+ ·73	"	"

New Haven, Sept. 1870.

ART. XLIII.—*Address of Thomas Henry Huxley, at the meeting of the British Association at Liverpool, on the 14th of Sept.*, 1870.*

IT has long been the custom for the newly installed President of the British Association for the Advancement of Science to take advantage of the elevation of the position in which the suffrages of his colleagues had, for the time, placed him, and, casting his eyes around the horizon of the scientific world, to report to them what could be seen from his watch-tower; in what directions the multitudinous divisions of the noble army of the improvers of natural knowledge were marching; what important strongholds of the great enemy of us all, ignorance, had been recently captured; and, also, with due impartiality, to mark where the advanced posts of science had been driven in, or a long-continued siege had made no progress.

I propose to endeavor to follow this ancient precedent, in a manner suited to the limitations of my knowledge and of my capacity. I shall not presume to attempt a panoramic survey of the world of scienee, nor even to give a sketch of what is doing in the one great province of biology, with some portions of which my ordinary occupations render me familiar. But I shall endeavor to put before you the history of the rise and progress of a single biological doctrine; and I shall try to give some notion of the fruits, both intellectual and practical, which we owe, directly or indirectly, to the working out, by seven generations of patient and laborious investigators, of the thought which arose, more than two centuries ago, in the mind of a sagacious and observant Italian naturalist.

It is a matter of every-day experience that it is difficult to prevent many articles of food from becoming covered with mould; that fruit, sound enough to all appearance, often contains grubs at the core; that meat, left to itself in the air, is apt to putrefy and swarm with maggots. Even ordinary water, if allowed to stand in an open vessel sooner or later becomes turbid and full of living matter.

The philosophers of antiquity, interrogated as to the cause of these phenomena, were provided with a ready and a plausible

* From "Nature," of Sept. 15.

answer. It did not enter their minds even to doubt that these low forms of life were generated in the matters in which they made their appearance. Lucretius, who had drunk deeper of the scientific spirit than any poet of ancient or modern times except Goethe, intends to speak as a philosopher, rather than as a poet, when he writes that "with good reason the earth has gotten the name of mother, since all things are produced out of the earth. And many living creatures, even now, spring out of the earth, taking form by the rains and the heat of the sun." The axiom of ancient science, "that the corruption of one thing is the birth of another," had its popular embodiment in the notion that a seed dies before the young plant springs from it; a belief so wide spread and so fixed, that Saint Paul appeals to it in one of the most splendid outbursts of his fervid eloquence:—"Thou fool, that which thou sowest is not quickened, except it die." *

The proposition that life may, and does, proceed from that which has no life, then, was held alike by the philosophers, the poets, and the people, of the most enlightened nations, eighteen hundred years ago; and it remained the accepted doctrine of learned and unlearned Europe, through the middle ages, down even to the seventeenth century.

It is commonly counted among the many merits of our great countryman, Harvey, that he was the first to declare the opposition of fact to venerable authority in this, as in other matters; but I can discover no justification for this wide-spread notion. After careful search through the "Exercitationes de Generatione," the most that appears clear to me is, that Harvey believed all animals and plants to spring from what he terms a "*primordium vegetale*," a phrase which may nowadays be rendered "a vegetative germ;" and this, he says, is "*oviforme*," or "egg-like;" not, he is careful to add, that it necessarily has the shape of an egg, but because it has the constitution and nature of one. That this "*primordium oviforme*" must needs, in all cases, proceed from a living parent is nowhere expressly maintained by Harvey, though such an opinion may be thought to be implied in one or two passages; while, on the other hand, he does, more than once, use language which is consistent only with a full belief in spontaneous or equivocal generation. In fact, the main concern of Harvey's wonderful little treatise is not with generation, in the physiological sense, at all, but with development; and his great object is the establishment of the doctrine of epigenesis.

The first distinct enunciation of the hypothesis that all living matter has sprung from pre-existing living matter, came from a contemporary, though a junior, of Harvey, a native of that country fertile in men great in all departments of human activity,

* 1 Corinthians, xv, 36.

which was to intellectual Europe, in the sixteenth and seventeenth centuries, what Germany is in the nineteenth. It was in Italy, and from Italian teachers, that Harvey received the most important part of his scientific education. And it was a student trained in the same schools, Francesco Redi—a man of the widest knowledge and most versatile abilities, distinguished alike as scholar, poet, physician, and naturalist—who, just two hundred and two years ago, published his "Esperienza intorno alla Generazione degl' Insetti," and gave to the world the idea, the growth of which it is my purpose to trace. Redi's book went through five editions in twenty years; and the extreme simplicity of his experiments, and the clearness of his arguments gained for his views, and for their consequences, almost universal acceptance.

Redi did not trouble himself much with speculative considerations, but attacked particular cases of what was supposed to be "spontaneous generation" experimentally. Here are dead animals, or pieces of meat, says he; I expose them to the air in hot weather, and in a few days they swarm with maggots. You tell me that these are generated in the dead flesh; but if I put similar bodies, while quite fresh, into a jar, and tie some fine gauze over the top of the jar, not a maggot makes its appearance, while the dead substances, nevertheless, putrefy just in the same way as before. It is obvious, therefore, that the maggots are not generated by the corruption of the meat; and that the cause of their formation must be a something which is kept away by gauze. But gauze will not keep away aëriform bodies, or fluid. This something must, therefore, exist in the form of solid particles too big to get through the gauze. Nor is one long left in doubt what these solid particles are: for the blowflies, attracted by the odor of the meat, swarm round the vessel, and, urged by a powerful but in this case misleading instinct, lay eggs out of which maggots are immediately hatched upon the gauze. The conclusion, therefore, is unavoidable; the maggots are not generated by the meat, but the eggs which give rise to them are brought through the air by the flies.

These experiments seem almost childishly simple, and one wonders how it was that no one ever thought of them before. Simple as they are, however, they are worthy of the most careful study, for every piece of experimental work since done, in regard to this subject, has been shaped upon the model furnished by the Italian philosopher. As the results of his experiments were the same, however varied the nature of the materials he used, it is not wonderful that there arose in Redi's mind a presumption, that in all such cases of the seeming production of life from dead matter, the real explanation was the introduction of living germs from without into that dead matter. And thus the hypothesis that living matter always arises by the agency of

pre-existing living matter, took definite shape; and had, henceforward, a right to be considered and a claim to be refuted, in each particular case, before the production of living matter in any other way could be admitted by careful reasoners. It will be necessary for me to refer to this hypothesis so frequently, that, to save circumlocution, I shall call it the hypothesis of *Biogenesis;* and I shall term the contrary doctrine—that living matter may be produced by not living matter—the hypothesis of *Abiogenesis.*

In the seventeenth century, as I have said, the latter was the dominant view, sanctioned alike by antiquity and by authority; and it is interesting to observe that Redi did not escape the customary tax upon a discoverer of having to defend himself against the charge of impugning the authority of the Scriptures; for his adversaries declared that the generation of bees from the carcase of a dead lion is affirmed, in the Book of Judges, to have been the origin of the famous riddle with which Samson perplexed the Philistines:—

> "Out of the eater came forth meat,
> And out of the strong came forth sweetness."

Against all odds, however, Redi, strong with the strength of demonstrable fact, did splendid battle for Biogenesis; but it is remarkable that he held the doctrine in a sense which, if he had lived in these times, would have infallibly caused him to be classed among the defenders of "spontaneous generation." "Omne vivum ex vivo," "no life without antecedent life," aphoristically sums up Redi's doctrine; but he went no further. It is most remarkable evidence of the philosophic caution and impartiality of his mind, that although he had speculatively anticipated the manner in which grubs really are deposited in fruits and in the galls of plants, he deliberately admits that the evidence is insufficient to bear him out; and he therefore prefers the supposition that they are generated by a modification of the living substance of the plants themselves. Indeed, he regards these vegetable growths as organs, by means of which the plant gives rise to an animal, and looks upon this production of specific animals as the final cause of the galls and of at any rate some fruit. And he proposes to explain the occurrence of parasites within the animal body in the same way.

It is of great importance to apprehend Redi's position rightly; for the lines of thought he laid down for us are those upon which naturalists have been working ever since. Clearly, he held *Biogenesis* as against *Abiogenesis;* and I shall immediately proceed, in the first place, to inquire how far subsequent investigation has borne him out in so doing.

But Redi also thought that there were two modes of Biogenesis. By the one method, which is that of common and ordi-

nary occurrence, the living parent gives rise to offspring which passes through the same cycle of changes as itself—like gives rise to like; and this has been termed *Homogenesis*. By the other mode the living parent was supposed to give rise to offspring which passed through a totally different series of states from those exhibited by the parent, and did not return into the cycle of the parent; this is what ought to be called *Heterogenesis*, the offspring being altogether, and permanently unlike the parent. The term Heterogenesis, however, has unfortunately been used in a different sense, and M. Milne-Edwards has therefore substituted for it *Xenogenesis*, which means the generation of something foreign. After discussing Redi's hypothesis of universal Biogenesis, then, I shall go on to ask how far the growth of science justifies his other hypothesis of Xenogenesis.

The progress of the hypothesis of Biogenesis was triumphant and unchecked for nearly a century. The application of the microscope to anatomy in the hands of Grew, Leeuwenhoek, Swammerdam, Lyonet, Vallisnieri, Reaumur, and other illustrious investigators of nature of that day, displayed such a complexity of organization in the lowest and minutest forms, and everywhere revealed such a prodigality of provision for their multiplication by germs of one sort or another, that the hypothesis of Abiogenesis began to appear not only untrue, but absurd; and, in the middle of the eighteenth century, when Needham and Buffon took up the question, it was almost universally discredited.

But the skill of the microscope-makers of the eighteenth century soon reached its limit. A microscope magnifying 400 diameters was a *chef d'œuvre* of the opticians of that day; and at the same time, by no means trustworthy. But a magnifying power of 400 diameters, even when definition reaches the exquisite perfection of our modern achromatic lenses, hardly suffices for the mere discernment of the smallest forms of life. A speck, only $\frac{1}{25}$th of an inch in diameter, has, at 10 inches from the eye, the same apparent size as an object $\frac{1}{10000}$th of an inch in diameter, when magnified 400 times; but forms of living matter abound, the diameter of which is not more than $\frac{1}{40000}$th of an inch. A filtered infusion of hay, allowed to stand for two days, will swarm with living things, among which, any which reaches the diameter of a human red blood-corpuscle, or about $\frac{1}{3200}$th of an inch, is a giant. It is only by bearing these facts in mind, that we can deal fairly with the remarkable statements and speculations put forward by Buffon and Needham in the middle of the eighteenth century.

When a portion of any animal or vegetable body is infused in water, it gradually softens and disintegrates; and, as it does so, the water is found to swarm with minute active creatures,

the so-called Infusorial Animalcules, none of which can be seen, except by the aid of the microscope; while a large proportion belong to the category of smallest things of which I have spoken, and which must have all looked like mere dots and lines under the ordinary microscopes of the eighteenth century.

Led by various theoretical considerations which I cannot now discuss, but which looked promising enough in the lights of that day, Buffon and Needham doubted the applicability of Redi's hypothesis to the infusorial animalcules, and Needham very properly endeavored to put the question to an experimental test. He said to himself, if these infusorial animalcules come from germs, their germs must exist either in the substance infused, or in the water in which the infusion is made, or in the superjacent air. Now the vitality of all germs is destroyed by heat. Therefore, if I boil the infusion, cork it up carefully, cementing the cork over with mastic, and then heat the whole vessel by heaping hot ashes over it, I must needs kill whatever germs are present. Consequently, if Redi's hypothesis hold good, when the fusion is taken away and allowed to cool, no animalcules ought to be developed in it; whereas, if the animalcules are not dependent on pre-existing germs, but are generated from the infused substance, they ought, by-and-by, to make their appearance. Needham found that, under the circumstances in which he made his experiments, animalcules always did arise in the infusions, when a sufficient time had elapsed to allow for their development.

In much of his work Needham was associated with Buffon, and the results of their experiments fitted in admirably with the great French naturalist's hypothesis of "organic molecules," according to which, life is the indefeasible property of certain indestructible molecules of matter, which exist in all living things, and have inherent activities by which they are distinguished from not living matter. Each individual living organism is formed by their temporary combination. They stand to it in the relation of the particles of water to a cascade, or a whirlpool; or to a mould, into which the water is poured. The form of the organism is thus determined by the reaction between external conditions and the inherent activities of the organic molecules of which it is composed; and, as the stoppage of the whirlpool destroys nothing but a form, and leaves the molecules of the water, with all their inherent activities intact, so what we call the death and putrefraction of an animal, or of a plant, is merely the breaking up of the form, or manner of association, of its constituent organic molecules, which are then set free as infusorial animalcules.

It will be perceived that this doctrine is by no means identical with *Abiogenesis*, with which it is often confounded. On

this hypothesis, a piece of beef, or a handful of hay, is dead only in a limited sense. The beef is dead ox, and the hay is dead grass; but the "organic molecules" of the beef or the hay are not dead, but are ready to manifest their vitality as soon as the bovine or herbaceous shrouds in which they are imprisoned are rent by the macerating action of water. The hypothesis therefore must be classified under Xenogenesis, rather than under Abiogenesis. Such as it was, I think it will appear, to those who will be just enough to remember that it was propounded before the birth of modern chemistry, and of the modern optical arts, to be a most ingenious and suggestive speculation.

But the great tragedy of Science—the slaying of a beautiful hypothesis by an ugly fact—which is so constantly being enacted under the eyes of philosophers, was played, almost immediately, for the benefit of Buffon and Needham.

Once more, an Italian, the Abbé Spallanzani, a worthy successor and representative of Redi in his acuteness, his ingenuity, and his learning, subjected the experiments and the conclusions of Needham to a searching criticism. It might be true that Needham's experiments yielded results such as he had described, but did they bear out his arguments? Was it not possible, in the first place, that he had not completely excluded the air by his corks and mastic? And was it not possible, in the second place, that he had not sufficiently heated his infusions and the superjacent air? Spallanzani joined issue with the English naturalist on both these pleas, and he showed that if, in the first place, the glass vessels in which the infusions were contained were hermetically sealed by fusing their necks, and if, in the second place, they were exposed to the temperature of boiling water for three-quarters of an hour,* no animalcules ever made their appearance within them. It must be admitted that the experiments and arguments of Spallanzani furnish a complete and a crushing reply to those of Needham. But we all too often forget that it is one thing to refute a proposition, and another to prove the truth of a doctrine which, implicitly or explicitly, contradicts that proposition, and the advance of science soon showed that though Needham might be quite wrong, it did not follow that Spallanzani was quite right.

Modern chemistry, the birth of the latter half of the eighteenth century, grew apace, and soon found herself face to face with the great problems which biology had vainly tried to attack without her help. The discovery of oxygen led to the laying of the foundations of a scientific theory of respiration, and to an examination of the marvellous interactions of organic substances with oxygen. The presence of free oxygen appeared to be one of the conditions of the existence of life, and of those

* See Spallanzani, "Opere," vi, pp. 42 and 61.

singular changes in organic matters which are known as fermentation and putrefaction. The question of the generation of the infusory animalcules thus passed into a new phase. For what might not have happened to the organic matter of the infusions, or to the oxygen of the air, in Spallanzani's experiments? What security was there that the development of life which ought to have taken place had not been checked or prevented by these changes?

The battle had to be fought again. It was needful to repeat the experiments under conditions which would make sure that neither the oxygen of the air, nor the composition of the organic matter, was altered in such a manner as to interfere with the existence of life.

Schulze and Schwann took up the question from this point of view in 1836 and 1837. The passage of air through red-hot glass tubes, or through strong sulphuric acid, does not alter the proportion of its oxygen, while it must needs arrest or destroy any organic matter which may be contained in the air. These experimenters, therefore, contrived arrangements by which the only air which should come into contact with a boiled infusion should be such as had either passed through red-hot tubes or through strong sulphuric acid. The result which they obtained was that an infusion so treated developed no living things, while if the same infusion was afterwards exposed to the air such things appeared rapidly and abundantly. The accuracy of these experiments has been alternately denied and affirmed. Supposing them to be accepted, however, all that they really proved was that the treatment to which the air was subjected destroyed, *something* that was essential to the development of life in the infusion. This "something" might be gaseous, fluid, or solid; that it consisted of germs remained only an hypothesis of greater or less probability.

Contemporaneously with these investigations a remarkable discovery was made by Cagniard de la Tour. He found that common yeast is composed of a vast accumulation of minute plants. The fermentation of must or wort in the fabrication of wine and of beer is always accompanied by the rapid growth and multiplication of these *Torulæ*. Thus fermentation, in so far as it was accompanied by the development of microscopical organisms in enormous numbers, became assimilated to the decomposition of an infusion of ordinary animal or vegetable matter; and it was an obvious suggestion that the organisms were, in some way or other, the causes both of fermentation and of putrefaction. The chemists, with Berzelius and Liebig at their head, at first laughed this idea to scorn; but in 1843, a man then very young, who has since performed the unexampled feat of attaining to high eminence alike in Mathematics, Physics,

and Physiology—I speak of the illustrious Helmholtz—reduced the matter to the test of experiment by a method alike elegant and conclusive. Helmholtz separated a putrefying or a fermenting liquid from one which was simply putrescible or fermentable by a membrane which allowed the fluids to pass through and become intermixed, but stopped the passage of solids. The result was, that while the putrescible or the fermentable liquids became impregnated with the results of the putrescence or fermentation which was going on on the other side of the membrane, they neither putrefied (in the ordinary way) nor fermented; nor were any of the organisms which abounded in the fermenting or putrefying liquid generated in them. Therefore the cause of the development of these organisms must lie in something which cannot pass through membranes; and as Helmholtz's investigations were long antecedent to Graham's researches upon colloids, his natural conclusion was that the agent thus intercepted must be a solid material. In point of fact, Helmholtz's experiments narrowed the issue to this: that which excites fermentation and putrefaction, and at the same time gives rise to living forms in a fermentable or putrescible fluid, is not a gas and is not a diffusible fluid; therefore it is either a colloid, or it is a matter divided into very minute solid particles.

The researches of Schroeder and Dusch in 1854, and of Schroeder alone, in 1859, cleared up this point by experiments which are simply refinements upon those of Redi. A lump of cotton-wool is, physically speaking, a pile of many thicknesses of a very fine gauze, the fineness of the meshes of which depends upon the closeness of the compression of the wool. Now, Schroeder and Dusch found, that, in the case of all the putrefiable materials which they used (except milk and yolk of egg), an infusion boiled, and then allowed to come into contact with no air but such as had been filtered through cotton-wool, neither putrefied nor fermented, nor developed living forms. It is hard to imagine what the fine sieve formed by the cotton-wool could have stopped except minute solid particles. Still the evidence was incomplete until it had been positively shown, first, that ordinary air does contain such particles; and, secondly, that filtration through cotton-wool arrests these particles and allows only physically pure air to pass. This demonstration has been furnished within the last year by the remarkable experiments of Professor Tyndall. It has been a common objection of Abiogenists that, if the doctrine of Biogeny is true, the air must be thick with germs; and they regard this as the height of absurdity. But Nature occassionally is exceedingly unreasonable, and Professor Tyndall has proved that this particular absurdity may nevertheless be a reality. He has demon-

strated that ordinary air is no better than a sort of stir-about of excessively minute solid particles; that these particles are almost wholly destructible by heat; and that they are strained off, and the air rendered optically pure by being passed through cotton-wool.

But it remains yet in the order of logic, though not of history, to show that among these solid destructible particles there really do exist germs capable of giving rise to the development of living forms in suitable menstrua. This piece of work was done by M. Pasteur, in those beautiful researches which will ever render his name famous; and which, in spite of all attacks upon them, appear to me now, as they did seven years ago,* to be models of accurate experimentation and logical reasoning. He strained air through cotton-wool, and found, as Schroeder and Dusch had done, that it contained nothing competent to give rise to the development of life in fluids highly fitted for that purpose. But the important further links in the chain of evidence added by Pasteur are three. In the first place he subjected to microscopic examination the cotton-wool which had served as strainer, and found that sundry bodies clearly recognizable as germs, were among the solid particles strained off. Secondly, he proved that these germs were competent to give rise to living forms by simply sowing them in a solution fitted for their development. And, thirdly, he showed that the incapacity of air strained through cotton-wool to give rise to life, was not due to any occult change effected in constituents of the air by the wool, by proving that the cotton-wool might be dispensed with altogether, and perfectly free access left between the exterior air and that in the experimental flask. If the neck of the flask is drawn out into a tube and bent downward; and if, after the contained fluid has been carefully boiled, the tube is heated sufficiently to destroy any germs which may be present in the air which enters as the fluid cools, the apparatus may be left to itself for any time, and no life will appear in the fluid. The reason is plain. Although there is free communication between the atmosphere laden with germs and the germless air in the flask, contact between the two takes place only in the tube; and as the germs cannot fall upward, and there are no currents, they never reach the interior of the flask. But if the tube be broken short off where it proceeds from the flask, and free access be thus given to germs falling vertically out of the air, the fluid which has remained clear and desert for months, becomes, in a few days turbid and full of life.

These experiments have been repeated over and over again by independent observers with entire success; and there is one

* "Lectures to Working Men on the Causes of the Phenomena of Organic Nature," 1863.

very simple mode of seeing the facts for oneself, which I may as well describe.

Prepare a solution (much used by M. Pasteur, and often called "Pasteur's solution") composed of water with tartrate of ammonia, sugar, and yeast-ash dissolved therein.* Divide it into three portions in as many flasks; boil all three for a quarter of an hour; and, while the steam is passing out, stop the neck of one with a large plug of cotton-wool, so that this also may be thoroughly steamed. Now set the flasks aside to cool, and when their contents are cold, add to one of the open ones a drop of filtered infusion of hay which has stood for twenty-four hours, and is consequently full of the active and excessively minute organisms known as *Bacteria*. In a couple of days of ordinary warm weather the contents of this flask will be milky from the enormous multiplication of *Bacteria*. The other flask, open and exposed to the air, will, sooner or later, become milky with *Bacteria*, and patches of mould may appear in it; while the liquid in the flask, the neck of which is plugged with cotton-wool, will remain clear for an indefinite time. I have sought in vain for any explanation of these facts, except the obvious one, that the air contains germs competent to give rise to *Bacteria*, such as those with which the first solution has been knowingly and purposely inoculated, and to the mould-*Fungi*. And I have not yet been able to meet with any advocate of Abiogenesis who seriously maintains that the atoms of sugar, tartrate of ammonia, yeast-ash, and water, under no influence but that of free access of air and the ordinary temperature, rearrange themselves and give rise to the protoplasm of *Bacterium*. But the alternative is to admit that these *Bacteria* arise from germs in the air; and if they are thus propagated, the burden of proof that other like forms are generated in a different manner, must rest with the assertor of that proposition.

To sum up the effect of this long chain of evidence:—

It is demonstrable that a fluid eminently fit for the development of the lowest forms of life, but which contains neither germs, nor any protein compound, gives rise to living things in great abundance if it is exposed to ordinary air, while no such development takes place if the air with which it is in contact is mechanically freed from the solid particles which ordinarily float in it, and which may be made visible by appropriate means.

It is demonstrable that the great majority of these particles are destructible by heat, and that some of them are germs or living particles, capable of giving rise to the same forms of life as those which appear when the fluid is exposed to unpurified air.

* Infusion of hay treated in the same way yields similar results; but as it contains organic matter, the argument which follows cannot be based upon it.

It is demonstrable that inoculation of the experimental fluid with a drop of liquid known to contain living particles, gives rise to the same phenomena as exposure to unpurified air.

And it is further certain that these living particles are so minute that the assumption of their suspension in ordinary air, presents not the slightest difficulty. On the contrary, considering their lightness and the wide diffusion of the organisms which produce them, it is impossible to conceive that they should not be suspended in the atmosphere in myriads.

Thus the evidence, direct and indirect, in favor of *Biogenesis* for all known forms of life must, I think, be admitted to be of great weight.

On the other side, the sole assertions worthy of attention are that hermetically sealed fluids, which have been exposed to great and long-continued heat, have sometimes exhibited living forms of low organization, when they have been opened.*

The first reply that suggests itself is the probability that there must be some error about these experiments, because they are performed on an enormous scale every day with quite contrary results. Meat, fruits, vegetables, the very materials of the most fermentable and putrescible infusions are preserved to the extent, I suppose I may say, of thousands of tons every year, by a method which is a mere application of Spallanzani's experiment. The matters to be preserved are well boiled in a tin case provided with a small hole, and this hole is soldered up when all the air in the case has been replaced by steam. By this method they may be kept for years without putrefying, fermenting, or getting mouldy. Now this is not because oxygen is excluded, inasmuch as it is now proved that free oxygen is not necessary for either fermentation or putrefaction. It is not because the tins are exhausted of air, for *Vibriones* and *Bacteria* live, as Pasteur has shown, without air or free oxygen. It is not because the boiled meats or vegetables are not putrescible or fermentable, as those who have had the misfortune to be in a ship supplied with unskillfully closed tins well know. What is it, therefore, but the exclusion of germs? I think that Abiogenists are bound to answer this question before they ask us to consider new experiments of precisely the same order.

And in the next place, if the results of the experiments I refer to are really trust-worthy, it by no means follows that Abiogenesis has taken place. The resistance of living matter to heat is known to vary within considerable limits, and to depend, to some extent, upon the chemical and physical qualities of the surrounding medium. But if, in the present state of

* For a full account of the most recent series of experiments of this description, see Dr. H. C. Bastian's paper in NATURE, No. xxxv, p. 170; No. xxxvi, p. 193; and No. xxxvii, p. 219.

science, the alternative is offered us, either germs can stand a greater heat than has been supposed, or the molecules of dead matter, for no valid or intelligible reason that is assigned, are able to rearrange themselves into living bodies, exactly such as can be demonstrated to be frequently produced in another way, I cannot understand how choice can be, even for a moment, doubtful.

But though I cannot express this conviction of mine too strongly, I must carefully guard myself against the supposition that I intend to suggest that no such thing as Abiogenesis ever has taken place in the past or ever will take place in the future. With organic chemistry, molecular physics, and physiology yet in their infancy, and every day making prodigious strides, I think it would be the height of presumption for any man to say that the conditions under which matter assumes the properties we call "vital" may not, some day, be artificially brought together. All I feel justified in affirming is that I see no reason for believing that the feat has been performed yet.

And looking back through the prodigious vista of the past, I find no record of the commencement of life, and therefore I am devoid of any means of forming a definite conclusion as to the conditions of its appearance. Belief, in the scientific sense of the word, is a serious matter, and needs strong foundations. To say, therefore, in the admitted absence of evidence, that I have any belief as to the mode in which the existing forms of life have originated, would be using words in a wrong sense. But expectation is permissible where belief is not; and if it were given me to look beyond the abyss of geologically recorded time to the still more remote period when the earth was passing through physical and chemical conditions, which it can no more see again than a man can recall his infancy, I should expect to be a witness of the evolution of living protoplasm from not living matter. I should expect to see it appear under forms of great simplicity, endowed, like existing fungi, with the power of determining the formation of new protoplasm, from such matters as ammonium carbonates, oxalates and tartrates, alkaline and earthy phosphates, and water, without the aid of light. That is the expectation to which analogical reasoning leads me; but I beg you once more to recollect that I have no right to call my opinion anything but an act of philosophical faith.

So much for the history of the progress of Redi's great doctrine of Biogenesis, which appears to me, with the limitations I have expressed, to be victorious along the whole line at the present day.

As regards the second problem offered to us by Redi, whether Xenogenesis obtains, side by side with Homogenesis; whether, that is, there exist not only the ordinary living things, giving

rise to offspring which run through the same cycle as themselves, but also others, producing offspring which are of a totally different character from themselves, the researches of two centuries have led to a different result. That the grubs found in galls are no product of the plants on which the galls grow, but are the result of the introduction of the eggs of insects into the substance of these plants, was made out by Vallisnieri, Reaumur, and others, before the end of the first half of the eighteenth century. The tapeworms, bladderworms, and flukes continued to be a stronghold of the advocates of Xenogenesis for a much longer period. Indeed, it is only within the last thirty years that the splendid patience of Von Siebold, Van Beneden, Leuckart, Küchenmeister, and other helminthologists, has succeeded in tracing every such parasite, often through the strangest wanderings and metamorphoses, to an egg derived from a parent, actually or potentially like itself; and the tendency of inquiries elsewhere has all been in the same direction. A plant may throw off bulbs, but these, sooner or later, give rise to seeds or spores, which develop into the original form. A polyp may give rise to Medusæ, or a pluteus to an Echinoderm, but the Medusa and the Echinoderm give rise to eggs which produce polyps or plutei, and they are therefore only stages in the cycle of life of the species.

But if we turn to pathology, it offers us some remarkable approximations to true Xenogenesis.

As I have already mentioned, it has been known since the time of Vallisnieri and of Reaumur, that galls in plants, and tumors in cattle, are caused by insects, which lay their eggs in those parts of the animal or vegetable frame of which these morbid structures are outgrowths. Again, it is a matter of familiar experience to everybody that mere pressure on the skin will give rise to a corn. Now the gall, the tumor, and the corn are parts of the living body, which have become, to a certain degree, independent and distinct organisms. Under the influence of certain external conditions, elements of the body, which should have developed in due subordination to its general plan, set up for themselves and apply the nourishment which they receive to their own purposes.

From such innocent productions as corns and warts, there are all gradations to the serious tumors which, by their mere size and the mechanical obstruction they cause, destroy the organism out of which they are developed; while, finally, in those terrible structures known as cancers, the abnormal growth has acquired powers of reproduction and multiplication, and is only morphologically distinguishable from the parasite worm, the life of which is neither more nor less closely bound up with that of the infested organism.

If there were a kind of diseased structure, the histological elements of which were capable of maintaining a separate and independent existence out of the body, it seems to me that the shadowy boundary between morbid growth and Xenogenesis would be effaced. And I am inclined to think that the progress of discovery has almost brought us to this point already. I have been favored by Mr. Simon with an early copy of the last published of the valuable "Reports on the Public Health," which, in his capacity of their medical officer, he annually presents to the Lords of the Privy Council. The appendix to this report contains an introductory essay "On the Intimate Pathology of Contagion," by Dr. Burdon Sanderson, which is one of the clearest, most comprehensive, and well-reasoned discussions of a great question which has come under my notice for a long time. I refer you to it for details and for the authorities for the statements I am about to make.

You are familiar with what happens in vaccination. A minute cut is made in the skin, and an infinitesimal quantity of vaccine matter is inserted into the wound. Within a certain time a vesicle appears in the place of the wound, and the fluid which distends this vesicle is vaccine matter, in quantity a hundred or a thousandfold that which was originally inserted. Now what has taken place in the course of this operation? Has the vaccine matter, by its irritative property, produced a mere blister, the fluid of which has the same irritative property? Or does the vaccine matter contain living particles, which have grown and multiplied where they have been planted? The observations of M. Chauveau, extended and confirmed by Dr. Sanderson himself, appear to leave no doubt upon this head. Experiments, similar in principle to those of Helmholtz on fermentation and putrefaction, have proved that the active element in the vaccine lymph is non-diffusible, and consists of minute particles not exceeding $\frac{1}{25000}$ of an inch in diameter, which are made visible in the lymph by the microscope. Similar experiments have proved that two of the most destructive of epizootic diseases, sheep-pox and glanders, are also dependent for their existence and their propagation upon extremely small living solid particles, to which the title of *microzymes* is applied. An animal suffering under either of these terrible diseases is a source of infection and contagion to others, for precisely the same reason as a tub of fermenting beer is capable of propagating its fermentation by "infection," or "contagion," to fresh wort. In both cases it is the solid living particles which are efficient; the liquid in which they float, and at the expense of which they live, being altogether passive.

Now arises the question, are these microzymes the results of *Homogenesis*, or of *Xenogenesis;* are they capable, like the *To-*

rulæ of yeast, of arising only by the development of preëxisting germs; or may they be, like the constituents of a nutgall, the results of a modification and individualization of the tissues of the body in which they are found, resulting from the operation of certain conditions? Are they parasites in the zoological sense, or are they merely what Virchow has called "heterologous growth?" It is obvious that this question has the most profound importance, whether we look at it from a practical or from a theoretical point of view. A parasite may be stamped out by destroying its germs, but a pathological product can only be annihilated by removing the conditions which give rise to it.

It appears to me that this great problem will have to be solved for each zymotic disease separately, for analogy cuts two ways. I have dwelt upon the analogy of pathological modification, which is in favor of the xenogenetic origin of microzymes; but I must now speak of the equally strong analogies in favor of the origin of such pestiferous particles by the ordinary process of the generation of like from like.

It is, at present, a well-established fact that certain diseases, both of plants and of animals, which have all the characters of contagious and infectious epidemics, are caused by minute organisms. The smut of wheat is a well-known instance of such a disease, and it cannot be doubted that the grape-disease and the potato-disease fall under the same category. Among animals, insects are wonderfully liable to the ravages of contagious and infectious diseases caused by microscopic *Fungi*.

In autumn, it is not uncommon to see flies, motionless upon a window-pane, with a sort of magic circle, in white, drawn round them. On microscopic examination, the magic circle is found to consist of innumerable spores, which have been thrown off in all directions by a minute fungus called *Empusa muscæ*, the spore-forming filaments of which stand out like a pile of velvet from the body of the fly. These spore-forming filaments are connected with others which fill the interior of the fly's body like so much fine wool, having eaten away and destroyed the creature's viscera. This is the full-grown condition of the *Empusa*. If traced back to its earlier stages, in flies which are still active, and to all appearance healthy, it is found to exist in the form of minute corpuscles which float in the blood of the fly. These multiply and lengthen into filaments, at the expense of the fly's substance; and when they have at last killed the patient, they grow out of its body and give off spores. Healthy flies shut up with diseased ones catch this mortal disease and perish like the others. A most competent observer, M. Cohn, who studied the development of the *Empusa* in the fly very carefully, was utterly unable to discover in what manner the

smallest germs of the *Empusa* got into the fly. The spores could not be made to give rise to such germs by cultivation; nor were such germs discoverable in the air, or in the food of the fly. It looked exceedingly like a case of Abiogenesis, or, at any rate, of Xenogenesis; and it is only quite recently that the real course of events has been made out. It has been ascertained, that when one of the spores falls upon the body of a fly, it begins to germinate and sends out a process which bores its way through the fly's skin; this, having reached the interior cavities of its body, gives off the minute floating corpuscles which are the earliest stage of the *Empusa*. The disease is "contagious," because a healthy fly coming in contact with a diseased one, from which the spore-bearing filaments protrude, is pretty sure to carry off a spore or two. It is "infectious" because the spores become scattered about all sorts of matter in the neighborhood of the slain flies.

The silkworm has long been known to be subject to a very fatal and infectious disease called the *Muscardine*. Audouin transmitted it by inoculation. This disease is entirely due to the development of a fungus, *Botrytis Bassiana*, in the body of the caterpiller; and its contagiousness and infectiousness are accounted for in the same way as those of the fly-disease. But of late years a still more serious epizoötic has appeared among the silkworms; and I may mention a few facts which will give you some conception of the gravity of the injury which it has inflicted on France alone.

The production of silk has been for centuries an important branch of industry in Southern France, and in the year 1853 it had attained such a magnitude that the annual produce of the French sericulture was estimated to amount to a tenth of that of the whole world, and represented a money-value of 117,000,000 of francs, or nearly five million sterling. What may be the sum which would represent the money-value of all the industries connected with the working up of the raw silk thus produced is more than I can pretend to estimate. Suffice it to say that the city of Lyons is built upon French silk as much as Manchester was upon American cotton before the civil war.

Silkworms are liable to many diseases; and even before 1853 a peculiar epizoötic, frequently accompanied by the appearance of dark spots upon the skin (whence the name of "Pébrine" which it has received), had been noted for its mortality. But in the years following 1853 this malady broke out with such extreme violence, that, in 1858, the silk-crop was reduced to a third of the amount which it had reached in 1853; and, up till within the last year or two, it has never attained half the yield of 1853. This means not only that the great number of people engaged in silk growing are some thirty millions sterling poorer than they might have been; it means not only that high prices

have had to be paid for importing silkworm eggs, and that, after investing his money in them, in paying for mulberry-leaves and for attendance, the cultivator has constantly seen his silkworms perish and himself plunged in ruin; but it means that the looms of Lyons have lacked employment, and that for years enforced idleness and misery have been the portion of a vast population which, in former days, was industrious and well to do.

In 1858 the gravity of the situation caused the French Academy of Sciences to appoint Commissioners, of whom a distinguished naturalist, M. de Quatrefages, was one to inquire into the nature of this disease, and, if possible, to devise some means of staying the plague. In reading the report* made by M. de Quatrefages in 1859, it is exceedingly interesting to observe that his elaborate study of the Pébrine forced the conviction upon his mind that, in its mode of occurrence and propagation, the disease of the silkworm is, in every respect, comparable to the cholera among mankind. But it differs from the cholera, and so far is a more formidable disease, in being hereditary, and in being under some circumstances, contagious as well as infectious.

The Italian naturalist, Filippi, discovered in the blood of the silkworms affected by the strange disease a multitude of cylindrical corpuscles, each about $\frac{1}{8000}$ of an inch long. These have been carefully studied by Lebert, and named by him *Pansistophyton;* for the reason that in subjects in which the disease is strongly developed, the corpuscles swarm in every tissue and organ of the body, and even pass into the undeveloped eggs of the female moth. But are these corpuscles causes, or mere concomitants, of the disease? Some naturalists took one view and some another; and it was not until the French Government, alarmed by the continued ravages of the malady, and the inefficiency of the remedies which had been suggested, dispatched M. Pasteur to study it, that the question received its final settlement; at a great sacrifice, not only of the time and peace of mind of that eminent philosopher, but, I regret to have to add, of his health.†

But the sacrifice has not been in vain. It is now certain that this devastating, cholera-like Pébrine is the effect of the growth and mutiplication of the *Panhistophyton* in the silkworm. It is contagious and infections because the corpuscles of the *Panhistophyton* pass away from the bodies of the diseased caterpillars, directly or indirectly, to the alimentary canal of healthy silkworms in their neighborhood; it is hereditary, because the corpuscles enter into the eggs while they are being formed, and consequently are carried within them when they are laid; and

* Etudes sur les Maladies Actuelles des Vers à Soie, p. 53.

† In Nature. No. xxxvi, p. 181, will be found a *résumé*, by Prof. Tyndall, of Pasteur's investigations of the silkworm disease.

for this reason, also, it presents the very singular peculiarity of being inherited only on the mother's side. There is not a single one of all the apparently capricious and unaccountable phenomena presented by the Pébrine, but has received its explanation from the fact that the disease is the result of the presence of the microscopic organism, *Panhistophyton*.

Such being the facts with respect to the Pébrine, what are the indications as to the method of preventing it? It is obvious that this depends upon the way in which the *Panhistophyton* is generated. If it may be generated by Abiogenesis, or by Xenogenesis, within the silkworm or its moth, the extirpation of the disease must depend upon the prevention of the occurrence of the conditions under which this generation takes place. But if, on the other hand, the *Panhistophyton* is an independent organism, which is no more generated by the silkworm than the mistletoe is generated by the oak or the apple-tree on which it grows, though it may need the silkworm for its development in the same way as the mistletoe needs the tree, then the indications are totally different. The sole thing to be done is to get rid of and keep away the germs of the *Panhistophyton*. As might be imagined, from the course of his previous investigations, M. Pasteur was led to believe that the latter was the right theory; and, guided by that theory, he has devised a method of extirpating the disease, which has proved to be completely successful wherever it has been properly carried out.

There can be no reason, then, for doubting that, among insects, contagious and infectious diseases, of great malignity, are caused by minute organisms which are produced from preëxisting germs, or by homogenesis; and there is no reason, that I know of, for believing that what happens in insects may not take place in the highest animals. Indeed, there is already strong evidence that some diseases of an extremely malignant and fatal character to which man is subject, are as much the work of minute organisms as is the Pébrine. I refer for evidence of this to the very striking facts adduced by Professor Lister in his various well-known publications on the antiseptic method of treatment. It seems to me impossible to rise from the perusal of those publications without a strong conviction that the lamentable mortality which so frequently dogs the footsteps of the most skillful operator, and those deadly consequences of wounds and injuries which seem to haunt the very walls of great hospitals, and are, even now, destroying more men than die of bullet or bayonet, are due to the importation of minute organisms into wounds, and their increase and multiplication; and that the surgeon who saves most lives will be he who best works out the practical consequences of the hypothesis of Redi.

I commenced this Address by asking you to follow me in an attempt to trace the path which has been followed by a scientific

idea, in its long and slow progress from the position of a probable hypothesis to that of an established law of nature. Our survey has not taken us into very attractive regions; it has lain, chiefly, in a land flowing with the abominable, and peopled with mere grubs and mouldiness. And it may be imagined with what smiles and shrugs, practical and serious contemporaries of Redi and of Spallanzani may have commented on the waste of their high abilities in toiling at the solution of problems which, though curious enough in themselves, could be of no conceivable utility to mankind. Nevertheless you will have observed that before we had traveled very far upon our road there appeared, on the right hand and on the left, fields laden with a harvest of golden grain, immediately convertible into those things which the most sordidly practical of men will admit to have value, viz: money and life.

The direct loss to France caused by the Pébrine in seventeen years cannot be estimated at less than fifty millions sterling; and if we add to this what Redi's idea, in Pasteur's hands, has done for the wine-grower and for the vinegar-maker, and try to capitalise its value, we shall find that it will go a long way towards repairing the money losses caused by the frightful and calamitous war of this autumn. And as to the equivalent of Redi's thought in life, how can we over-estimate the value of that knowledge of the nature of epidemic and epizoötic diseases, and consequently of the means of checking, or eradicating, them, the dawn of which has assuredly commenced?

Looking back no further than ten years, it is possible to select three (1863, 1864, and 1869) in which the total number of deaths from scarlet-fever alone amounted to ninety thousand. That is the return of killed, the maimed and disabled being left out of sight. Why, it is to be hoped that the list of killed in the present bloodiest of all wars will not amount to more than this! But the facts which I have placed before you must leave the least sanguine without a doubt that the nature and the causes of this scourge will, one day, be as well understood as those of the Pébrine are now; and that the long-suffered massacre of our innocents will come to an end.

And thus mankind will have one more admonition that "the people perish for lack of knowledge;" and that the alleviation of the miseries, and the promotion of the welfare, of men must be sought, by those who will not lose their pains, in that diligent, patient, loving study of all the multitudinous aspects of Nature, the results of which constitute exact knowledge, or Science. It is the justification and the glory of this great meeting that it is gathered together for no other object than the advancement of the moiety of science which deals with those phenomena of nature which we call physical. May its endeavors be crowned with a full measure of success.

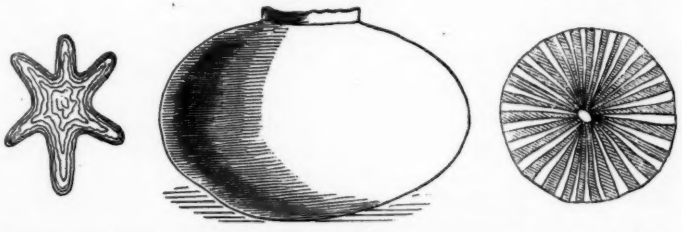
ART. XLIV. — *The Hail-storm of June 20th*, 1870; by Rev. HORACE C. HOVEY, M.A.

THIS remarkable storm swept along a path about thirty miles wide, and extending from Troy, N. Y., to Bangor, Me., though it was not everywhere accompanied by hail.

My point of observation was in Northampton, Mass., which was in the central line of the storm.

At sunrise the atmosphere was obscured by fog, which was partially dispersed at a later hour. The day was sultry. At noon the thermometer indicated 88° in the shade. At 3 P. M. a vast mass of dark-green cloud rolled up from the N. W., while lateral currents seemed to set in, forcing the clouds at first into confusion, but afterwards into a well-defined vortex, or spout. The electrical detonations were frequent and sharp. No rain preceded the hail, though it fell copiously after a few minutes. The first hail-stones were about one inch in diameter, and seemed to fall from a greater height, and with more force, than those that fell subsequently. The latter were probably nearer the center of the vortex, and so had their downward motion restrained by that which was lateral. The first that fell were, most of them, on striking the ground, instantly buried out of sight. If they struck on a rocky surface they were dashed in pieces, or else rebounded to a considerable height in in the air. Had their larger successors been driven by a corresponding force, nothing could have survived their assault. The smaller hail-stones were generally flattened spheres, though sometimes in rude stellar forms, (fig. 1). But the largest ones were symmetrical ovoids; each being surmounted, however, by a roughened crown, (fig. 2). The dimensions and weight of

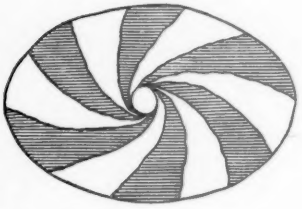
1. 2. $3\frac{1}{4}'' \times 2\frac{1}{4}''$. 3. 2″ diam.



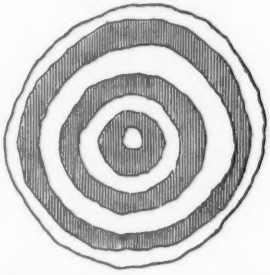
three specimens are given, with such accuracy as could be secured by the means at hand. These are but samples of thousands that fell till the earth was covered with ice. The first was, in long diameter, $3\frac{6}{8}$ inches; short diameter, $2\frac{1}{8}$ inches; weight 7 ounces. The second was $3\frac{1}{2}$ inches by $2\frac{1}{4}$; weight 8 ounces. The third was 4 inches by $2\frac{6}{8}$; weight 10 ounces.

This monster, a foot in circumference, did not entirely melt away for six hours after it fell! The ice in all the hail-stones was peculiarly hard and compact. Interesting structural peculiarities were noted. Hail-stones of stellar form were always transparent and homogeneous. The spheroids were covered with an opaque coating, and had likewise an opaque center. On being bisected some of them showed a radiated structure, the alternate rays being white and clear, (fig. 3). The largest hail-stones had an axis of white ice, half an inch in diameter, around which the alternate layers were arranged in spiral con-

4. $3\frac{1}{2}'' \times 2\frac{1}{2}''$.



5. 3″ diam.



volutions, (fig. 4). The most common form was in concentric layers, like the coats of an onion, still alternating opaque and transparent; but the edges were finely serrated, like the stripes in some species of agate, (fig. 5). In one hailstone I counted thirteen of these layers, indicating that it had passed through as many strata of snowy and vaporous cloud.

After a lull in the storm, for half an hour, there was a second fall of hail, but much lighter than the first.

The damage done by such a war of the elements cannot easily be ascertained. Vegetation suffered greatly. In some cases men and animals were wounded. The icy missles not only broke thousands of pains of glass, but also in many instances the window-blinds and sash. In a few cases weather-worn house-roofs were pierced.

Peoria, Ill., July 25, 1870.

ART. XLV.—*Photograph of a Solar Prominence;* by Prof. C. A. YOUNG, of Dartmouth College.

THE following is from a letter to the editors dated Sept. 28th, 1870:—

I have just succeeded, with the help of our skillful artist Mr. H. O. Bly, in obtaining a photograph of one of the solar prominences, a copy of which I enclose. It was taken through the

hydrogen line, near G, by opening the slit of the spectroscope and attaching a small camera to its eye-piece. As a *picture* of course it amounts to very little. It required an exposure of three minutes and a half, and the polar axis of the telescope being imperfectly adjusted, the clock-work failed to follow perfectly, so that no detail is visible, and the picture will not bear much magnifying. I am convinced, however, that by using a more sensitive collodion, and taking proper pains with the adjustment of the instrument, satisfactory photographs of these curious objects may be obtained.

I may add that the spectroscope employed has the dispersive power of 13 prisms of flint, each with an angle of 55°.

With it I observed this afternoon in the spectrum of a spot, the reversal of the following lines, viz: C, D_1, D_2, D_3, 1474 K, (very faint), b_1, b_2, b_3, b_4, F, 2796 K, ($H\gamma$), and h ($H\delta$). b_3 was most conspicuous after C_1, D_3, and F.

ART. XLVI.—*Contributions to Zoölogy from the Museum of Yale College.* No. 8.—*Descriptions of some New England Nudibranchiata;* by A. E. VERRILL.

DURING a dredging expedition to Eastport, Me., and Grand Menan, the past season, in company with Mr. Oscar Harger and C. H. Dwinelle, students in the Sheffield Scientific School, the following very interesting species was obtained. Many other Nudibranchs were also observed, most of which are well known species.

Dendronotus robustus, sp. nov. Figure 1.

Body stout, about 2 inches long; ·5 broad, and about the same in height, somewhat quadrangular, tapering posteriorly, but much less acute than in *D. arborescens*, as well as much stouter throughout. Branchiæ in about six pairs, those of the three first pairs with a supplementary one of nearly the same size arising separately outside of, but close to their bases; on the fourth pair these originate from the base as large branches, and on the following ones they are more distinctly branches, arising from the sides near the bases of the branchiæ. The branchiæ are diffusely arborescent and very much subdivided, the divisions taking place very rapidly, the branches being more equal in length and more spreading than in *D. arborescens*, and do not have the long, slender and acute main branches seen in that species. The sheaths of the tentacles (figure 1, *a*) are round and stout, about ·4 of an inch long and ·12 in diameter, and are destitute of any lateral branches; they divide at top into five simple, round, smooth, tapering, acute divisions, of which the two posterior ones are longest. The tentacles (figure

1, *b*) are about equal in length to the lobes of the sheath, the pedicle forming about half of the visible part; the terminal portion suddenly enlarges at first, becomes somewhat conical, and tapers to an obtuse point; it has ten or twelve oblique plications. Front of head with numerous (about thirty) sparingly branched appendages arranged in two series. In the upper series there are about ten, the outer ones being largest; these have stout stems with a few conical, tapering branches, mostly on the lower side, which are tipped with sulphur-yellow. Below these are numerous unequal, smaller, and more simple appendages, about ten on each side, part of which are forked at the end, while others are simple and papilliform and surround the expanded oral disk; all are tipped with yellow. The oral disk (figure 1, *c*) is tranversely elliptical. The foot is nearly as broad as the body (·4 of an inch), and can be adapted for clasping by infolding the edges.

Color pale grayish, thickly sprinkled with small yellow spots, which become less numerous on the oral appendages and sheaths of the tentacles.

Whale Cove, Grand Menan, on sea-weeds in a pool near low-water mark. One specimen only, found by Mr. Oscar Harger.

Dendronotus arborescens Alder and Hancock (*D. Reynoldsii* Couthouy) differs widely from this species in having a very narrow foot; an elevated compressed body, which is more slender and more acute behind; a much smaller number (about ten or twelve) appendages in front of the head, of which the six upper ones are larger and much more branched, and the four lower ones very small; the gills longer and the branches more unequal, while the lowest branch on the outside arises from the side, above the base, even on the front pairs; and in having more clavate tentacles, with longer and branched lobes to their sheaths, while the sheaths also have a large, arborescent, gill-like branch originating from the outer side toward the base. By the last character alcoholic specimens can easily be distinguished. Both species occurred together in the same pool.

Doris bifida, sp. nov.

Outline broad oval, widest anteriorly, 1 inch long by ·5 broad, in extension, back very convex, mantle covered with numerous, scattered, prominent, pointed papillæ. Tentacles rather long, thickest in the middle, the outer half strongly plicated, but with a smooth tip, the base surrounded by small papillæ. Gills retractile into a single cavity, united together by a partial web,

Figure 1.—*Dendronotus robustus* Verrill; *a*, tentacle sheath, natural size; *b*, tentacle, enlarged; *c*, oral disk and anterior part of the foot, natural size; from living specimen by A. E. Verrill.

deeply frilled, much subdivided, bipinnate, the subdivisions fine and slender. Foot very broad, in extension projecting back beyond the mantle about a quarter of an inch, slightly tapering, rounded and slightly notched at the end. Oral disk or veil crescent-shaped, the front a little prominent, the sides extended backward and forming a curve continuous with that of the foot.

Color dark purplish brown, sprinkled with white specks; tentacles deep brown, specked with white, tips yellowish; gills purplish at base, the edges and tips yellow; foot similar in color to mantle, but lighter.

Eastport, Me., at low-water mark, under stones, Aug. 19, 1868.

Onchidoris tenella.

Doris tenella Ag., in Gould, Invert. of Mass., Ed. 2, p. 229, Pl. xx, figures 289, 290, 293, 1870.

Specimens of this rare and imperfectly known species were obtained under stones in a large pool at low-water mark, near Eastport, Me.

The largest one was ·35 of an inch long, and ·20 to ·25 broad, according to the position. The outline is oval, elliptical, or oblong, in different states of extension, and the edges of the mantle are often rolled inward. The back is strongly convex, the surface thickly covered with small conical papillæ, which are strengthened by numerous minute white spicula. The tentacles are rather long, oblong, scarcely tapering, with numerous transverse laminæ, which cover nearly the whole length, the tip with a small obtuse papilla; the base is surrounded by a short sheath, with the edge divided into five small conical papillæ or teeth, the two anterior ones largest. Branchiæ nine, seven principal ones with two very small ones posteriorly; the larger ones are short, thick, lanceolate, with short lateral lobes. In the center of the branchial circle there is a small brownish papilla. The foot is long-oval, tapering behind and rounded in front, about half as wide as the mantle, and very much shorter. The oral disk is short and broad, subtriangular, with a very obtuse angle in front.

Color of the upper surface yellowish white, the papillæ mostly tipped with yellow, but some with flake-white; tentacles lemon-yellow with lighter tips; branchiæ yellowish white, edged and tipped with lemon-yellow, the yellow tint conspicuous in partial contraction; foot yellowish white; mouth and edge of oral disk bright yellow.

Onchidoris grisea.

Doris grisea Stimpson MS., in Gould, op. cit., p. 232, Pl. xx, figures 292, 295.

This species occurred under the same circumstances as the preceding, and more commonly. The color was generally clear white, sometimes tinged with pale sulphur-yellow, in some parts.

Onchidoris pallida.

Doris pallida Ag., in Gould, op. cit., p. 229, Pl. xx, figs. 284, 287, 288, 291.

This species was dredged in 20 fathoms in Eastport harbor. It has much larger tubercles than either of the preceding.

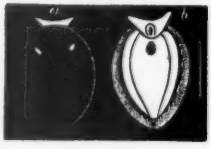
Doridella, gen. nov.

Body covered with an ample, smooth mantle, oval, convex. Dorsal tentacles retractile, without sheaths. Head prominent, the lateral angles prolonged anteriorly as short oral palpi or tentacles. Foot broad, cordate. Branchiæ posterior, in the groove between the mantle and foot.

Doridella obscura, sp. nov. Figures 2 and 3.

Form broad oval, ·3 of an inch long and ·2 broad; back convex, smooth. Foot broad, cordate in front. Oral disk broad, emarginate or with concave outline in front; The angles somewhat produced, forming short, tentacle-like organs, which in extension project beyond the front edge of the mantle. Dorsal tentacles small, stout, retractile. Color of body blackish, lighter toward the edge, as if covered with nearly confluent black spots, the whitish ground color showing between them; foot, oral disk, and dorsal tentacles white; the central part of body, beneath, bright yellow.

2.



Savin Rock, near New Haven, Oct. 28, 1868,—E. T. Nelson.

The eggs, laid in confinement, were very small, pale yellow, numerous, arranged in an open coil, (figure 3).

3.

This is the only Nudibranch hitherto discovered in the vicinity of New Haven. It appears to be allied to *Phyllidia* and *Fryeria*, which are usually referred to the Tectibranchs.

ART. XLVII.—*On a recent Earthquake at Bogota;* by the Hon. S. A. HURLBUT, U. S. Minister to Columbia.

[The following communication respecting an earthquake recently felt at Bogota, observed by the Hon. S. A. Hurlbut, U. S. Minister to Columbia, has been kindly furnished to us by the Secretary of the Smithsonian Institution, to whom it was addressed.]

WE have had rather an unusual phenomenon at this place in a remarkably well developed earthquake. At about 10 minutes before 10 P. M. of the evening of the 4th of April (Saturday), and without any previous warning that we had noticed, there

Figure 2.—*Doridella obscura* Verrill, enlarged two diameters; *a*, upper surface; *b*, lower surface.

Figure 3.—Eggs of *D. obscura*, enlarged two diameters.

occurred first, a moderate shock not of any peculiar force and consisting of a single vibration; this was momentary. In about two minutes afterward a very sharp movement took place, giving the impression of a lateral motion from north to south. The table on which I had my elbow at the time seemed to recede about $1\frac{1}{2}$ to 2 inches, quiver an instant and return to its place: the beams of the houses creaked like the timbers of a ship in heavy weather. Doors and windows flew open. Those who were in bed at the time seemed to feel it much more, and the effect of the vibrations was to make many "sea sick." This shock, they tell me, was the sharpest known here since 1826. I cannot learn of any damage done to buildings in the city.

The unquietness of the earth continued from the time mentioned until nearly 11 P. M., with a species of shuddering motion scarcely perceptible unless one were lying down. There was heard with each shock, a peculiar muffled rushing sound, not as clear and distinct as the movement of wind, but something like it. At the moment of the principal shock I looked at my watch and found the time to be ten minutes of ten—Bogota time. Time however, here, is not well regulated, as the observatory possesses no instruments and is neglected. The direction of the movement was very distinct from the north to the south. As earthquakes rarely have their centers in Columbia and are generally the result of action in Ecuador, it may be advisable to connect this observation with notices from that country. I believe there is but one volcano in action in Colombia-Puracè.

Some nights since we noticed for two hours after sunset in the west, and nearly in the range of Tolima, a well defined column or line of light, on the Cordillera. This bore about due west. The character of the light I could not determine.

Legation of the United States, Bogota, June 6, 1870.

ART. XLVIII.—*Discovery of a new Planet, the* 112*th, named Iphigenia;* by Dr. C. H. F. PETERS, of the Litchfield Observatory of Hamilton College. Letter to the Editors, dated Clinton, Oneida Co., N. Y., Sept. 22, 1870.

I HAVE the pleasure to communicate the following observations upon an asteroid discovered on the night of the 19th inst.

1870	Ham. Coll. m.t.	App. A.R.	App. Decl.	
	h m	h m s	° ′	
Sept 19.	15 30 —	1 2 35	+10 16 —	(approx. by estimation).
" 20.	14 57 28	1 1 51·54	+10 13 30·8	10 comp. W. 0^h 1079.
" 21.	10 48 22	1 1 12·24	+10 10 58·9	10 comp. Sohj. 374.

The planet is about 11th magnitude, receives the number (112), and I have already given a name to it, *Iphigenia*, while that found on Aug. 14, (111), has been called *Ate*, with regard to the simultaneous events in Europe.

ART. XLIX.—*Geological Explorations in China;* by Baron von RICHTHOFEN. In a letter to Prof. J. D. WHITNEY, dated Peking, Aug. 20th, 1869, and communicated by him for this Journal.

I PROMISED you, a few days ago, a more detailed account of the geological results of my travels in Manchuria and the province of Chi-li than I wished to give before having visited at least some of the localities near Peking examined before by Pumpelly. I have done this and am now acquainted with the most important formations occurring in the neighborhood of this capital.

The southern province of Manchuria has the name Shing-King and is divided by the Liao river into Liao-tung and Liao-hsi (meaning East and West of the Liao). The course of my travels from May to July was as follows: from Chifu by sea to Nin-chang, at the mouth of the Liao; thence by land down the western and up the southeastern coast of Liao-tung, to the frontier of Corea, then to the northeast, along this frontier, and to Mukden; from Mukden I went to Peking by Kin-chan and Yung-ping.

There are certain circumstances which render the geology of northern China difficult. In the first place the extent of the country is very great, and the character of the formations changes no less in the different provinces of it than it does in other regions of the globe. One is easily inclined, on a hasty tour of reconnoissance, to compare the strata in various regions on lithological grounds. But if I bear in mind the erroneous conclusions arrived at in the European Alps, by geologists who endeavored to determine the age of the sedimentary formations on the strength of their lithological resemblance to the formations of other parts of Europe, I think I cannot be cautious enough in this new country. Lithological analogy can here be used as a safe guide only when carefully traced from province to province and so on to remote regions. Then there is the apparent absence of any great geological events creating disturbances simultaneously over the whole region. Ancient deposition continued in one place while it was interrupted in another by the dislocation of those strata previously deposited. Another difficulty is the scarcity of fossils. I dare say that, with the exception of a few plants of the Coal-measures, I have discovered all localities of fossils now known to exist. Yet, if it is considered that I never received any knowledge of their existence (excepting Lake Tai-hu) not even the slightest hint, from either native or foreigner, but that I had to discover every fossil myself in hurrying through the country, it will still be found surprising, that the number of known fossiliferous localities is so great. I believe that China will, on a closer examination, contribute largely to the knowledge of the most ancient animal life on the globe. There is another difficulty caused by the recurrence, at different levels, of strata which bear a close similarity to each other. This relates chiefly to certain quartzose sandstones of a

reddish or yellow color when converted into quartzite. They have often a thickness of thousands of feet, and, as they are less affected by denudation than many other stratified rocks, they frequently compose mountain ranges for themselves, without offering any clue for determining their stratigraphical position. This is also true, though in a far less degree, of the limestones. In regard to these, as also to the knowledge of the occurrence of fossils, the analogy with the history of the exploration of the European Alps is striking. One cannot be astonished that the first explorer of Chinese geology, though a most admirable observer, should have distinguished only one great limestone formation in China, and referred it all to that age (namely, the Devonian) to which all the fossils then known (namely, those which are sold in drug stores) were believed to belong; just as "the Alpine limestone" was until not very long ago a comprehensive term used to designate all the limestones occurring at different geological levels in the Alps. You may recollect that already my observations on the Yang-tse induced me to distinguish there at least three limestone formations. Since then I have come to the conclusion that it will not be difficult, on detailed examination, to establish in other regions a greater number at distinct levels.

It is for the various reasons mentioned, that I examined every mountainous country independently from what I had seen before, applying new terms for the different formations observed, and tried to establish the analogy between different regions only after having completed the exploration of each. The series of formations, as established on the Yang-tse, has found thereby a great deal of additional support and, I think, will prove to be a near approximation to the true order of succession.

In my two previous letters (March 1st and May 8th) I mentioned my having found Carboniferous fossils on the Yang-tse and in Shantung, in certain limestones and shales which occur there in close connection with the Coal-measures, or indeed form part of them. They mark there a very distinct and remarkable horizon. I did not find any corresponding fossils in Manchuria or Chili. There, however, I was fortunate enough to collect sufficient material for determining the age of a formation which corresponds, stratigraphically and lithologically, with the lowest ("Matsu") limestone on the Yang-tse and the formation mentioned as No. 2 of Shantung in my letter of May 8th. It has a thickness of many thousand feet, and is, lithologically, exceedingly varied. It consists of endless alternations of red micaceous argyllite and brown sandstone, with strata of limestone, the former two prevailing in the lower half, the latter in the upper portion. The limestone itself presents a large number of varieties, and one can distinguish, on purely lithological grounds, several horizons, the order of succession of which is similar in wide regions. One of these horizons is marked by black oölitic limestone, which, together with some other conspicuous varieties of limestone, extends from the Yang-tse to Liao-tung and to Peking (twelve degrees of latitude and ten of longitude).

On the frontier of Corea, this oolitic limestone abounds in the remains of Trilobites and small brachiopods, chiefly of the Orthis and Lingula families. The sediments were there deposited in inlets of the ancient sea between ridges consisting chiefly of gneiss, granite and quartzite. The accumulation of the shells of trilobites which took place in these protected abodes is astonishing. I collected quite a number of species, but nothing adequate to the material which I saw, as several circumstances, chiefly the danger of being cut off by torrents swelled by the copious rains of the season, made it necessary to hurry over the most prolific localities. Yet, I hope that it will now be possible to determine the age of one of the most important formations of Northern China. It predominates, in bulk, over all other sedimentary formations of Lio-tung and extends thence into Corea. In Liao-tsi, it appears to have been removed by denudation to a great extent. It appears only in places, and even in these it is often covered by porphyries. Again it takes a dominent part in the structure of the country in the province of Chi-li, chiefly between Yung-ping-pu and Peking, becoming, however, more and more metamorphosed with the approach to the latter city. Its limestones (that is, its upper portion) form the narrows which lead to the Nankan pass. They are intensely altered, and traversed by innumerable dykes of intrusive rocks. What Pumpelly describes as the Hwaingan beds, are probably the lower strata of this same great formation.

In Liao-tung, as in Shan-tung, the oolitic limestones carrying the trilobites, are overlain by a great thickness of limestone, which is immediately followed by the Coal-measures. In Liao-tung, I found no fossils in this limestone, excepting numerous Ammonites and Orthoceratites, which cannot be determined. They occur immediately below the coal-bearing strata, the conformable superposition of which on the limestone I observed in a number of places. There are localities, such as northwestern Shan-tung and southern Liao-tung, where there is an apparently uninterrupted series of sedimentary deposits, commencing with sandstones and shales thousands of feet lower than the oolitic limestone and ending with the Coal-measures. Every layer appears to be conformable to that which it overlies, but no such parallelism exists between the lower and upper portions of the series, a gradual change of inclination marking the former gradual changes of level. It is not improbable that in these localities the entire series of formations, from the Silurian to the Coal-measures, is represented. It is in these regions that the order of succession will have to be studied. There are, however, more numerous instances where the outbreaks first of granitic and then of porphyric rocks, which extended evidently over long periods, have created repeated disturbances. The vicinity of Peking has been among the theaters of the most intense eruptive action. But even here the Coal-measures are conformable to the underlying limestones, in all instances which came under my observation.

I am unable to pronounce at present an opinion on the relation of the northern Coal-measures to those of Shan-tung and the lower Yang-tse. The remains of plants which I collected in nearly every one of the northern coal-fields will probably help to elucidate this question. But, judging on mere stratigraphical grounds, I cannot help thinking, that the Coal-measures of Liao-tung and Liao-hsi, and the lower portion of those in the neighborhood of Peking, will not differ much in age, either among themselves, or as compared with the Coal-measures of Middle China, It is, however, a noteworthy fact, that the coal formation of Peking has an extraordinary development. I had only occasion to see those coal-bearing strata which lie immediately on the limestone. West of Peking, these strata, together with the limestone and a few thousand feet of superincumbent deposits, have undergone an intense metamorphism, the coal being converted into anthracite. It appears, from Mr. Pumpelly's description, that the Chaitung Coal-measures (which I did not visit) occupy a, geologically, much higher level. Altogether it may be safe to conclude, that in China, as in other countries, the deposition of coal and intervening sediments continued during a considerable period, in which it shifted to different regions. Yet, I must confess, that comparison on stratigraphical grounds makes it difficult to believe that any portion of the Coal-measures of Northern China should be of so recent age as Dr. Newberry was inclined to conclude on the strength of the vegetable remains which he determined.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *On the influence of electricity on air and oxygen as a means of producing ozone.*—HOUZEAU has drawn the following conclusions from a great number of estimations of ozone obtained by means of Ruhmkorff's apparatus.

(1.) The production of ozone is greater in air renewed from time to time than in confined air.

(2.) It is greater at the negative than at the positive pole.

(3.) The production of ozone increases only up to a certain point with the duration of the electric action.

(4.) The ozone increases with the electric intensity.

(5.) The ozone diminishes when the distance which separates the electrodes increases.

(6.) The production of ozone varies with the length or surface of the electrodes.

(7.) Other conditions being equal, the production of ozone is greater by utilizing the effect of the two electrodes.

(8.) The production of ozone is equally manifested, out of direct contact with the air, with metallic electrodes, when these last are surrounded for their whole lengths with tubes of thin glass play-

ing the part of insulating sheaths, whether the extremities of these tubes are closed or not.

(9.) Still the production of ozone resulting from the passage of air over the naked metallic electrodes (direct contact with the platinum wires) is greater than that which arises from the passage of the air round the same electrodes, when sheathed and closed, (no direct contact of the air with the naked metallic electrodes).

(10.) With closed sheathed electrodes the production of ozone varies equally with the length or surface of the metallic electrodes.

(11.) The production of ozone increases considerably with a diminution of the temperature at which the electrification of the air is effected.

(12.) All conditions being equal, the quantity of ozone produced with a definite volume of oxygen, is always much more considerable (about eight or ten times) than that furnished by the same volume of air.

(13.) The ozone produced by the obscure electrification of air, is accompanied by small quantities of nitrous compounds, while that which is furnished by pure oxygen under the same circumstances, contains only traces. By attention to the conditions above described, the author has been able to construct a new apparatus, which he calls an *ozonizer*, and with which, according to his statement, quantities of ozone hitherto unknown, may be prepared. The apparatus is not described in the paper from which our extract is taken.— *Comptes Rendus*, lxx, 1286. W. G.

2. *Researches on Platinum.*—SCHÜTZENBERGER has communicated to the Academy of Sciences further investigations of the remarkable compounds of platinum with carbonic oxyd, already noticed in this Journal, and has in addition described some new series of great interest and theoretical importance. Setting out with the view that the two compounds already described, $CO = PtCl_2$, and $\begin{matrix} CO \\ | \\ CO \end{matrix} > PtCl_2$,* may be regarded as the chlorids of two diatomic radicals, the author found that ammonia unites with each compound, forming the two new chlorids represented respectively by the formulas,

$$N_2H_6 . CO . PtCl_2, \text{ and } N_2H_6 . (CO)_2 . PtCl_2.$$

When heated these compounds fuse, yielding metallic platinum, nitrogen, hydrogen and sal-ammoniac, together with a volatile liquid having a penetrating odor which appears to be chlorid of formyl, COHCl. The compound $CO . PtCl_2$ absorbs dry ethylene and forms a yellow crystalline body, which has probably the formula $C_2H_4 . CO . PtCl_2$, and may be regarded as corresponding to the dicarboxyl compound $(CO)_2 . PtCl_2$, C_2H_4 replacing CO. When heated above 95° C. this body gives off chlorhydric acid, while a dark colored body remains insoluble in water and having the formula $C_2H_3Cl . CO . PtCl_2$, so that it contains monochlorinated ethylene in place of ethylene.

Phosphorous chlorid, PCl_3, like carbonic oxyd, unites readily

* Pt=197 C=12 O=16.

with platinous chlorid, forming a solid reddish-yellow substance, volatile with difficulty, fusing at about 200°, and soluble in phosphorous chlorid, from which it crystallizes in cooling. The composition of this body is represented by the formula $PCl_3 . PtCl_2$. The same substance is formed by heating one atom of platinum with one molecule of phosphoric chlorid, $PCl_5 + Pt = PCl_3 . PtCl_2$, and also by the action of PCl_3 upon $CO . PtCl_2$, CO being given off. The compound $PCl_3 . PtCl_2$ dissolves in water with a yellow color, forming a new complex acid, $P(HO)_3 PtCl_2$, and differs essentially from the body described by Baudrimont, which according to that chemist, has the formula $PCl_5 . PtCl_2$, (Pt=98·5). In a later communication to the Academy, Schützenberger gives the formulas and names of a series of very remarkable substances which he has obtained from the primitive, $PCl_3 . PtCl_2$; these are as follows:

$PCl_3 . PtCl_2$ chlorid of trichloro-phosphoplatinum.
$P(HO)_3 . PtCl_2$ phospho chloro-platinous acid.
$P(A_gO)_3 . PtCl_2$ silver salt of the same.
$P(C_2H_5O)_3 . PtCl_2$ chlorid of trioxethyl-phospho platinum.
$P(C_2H_5O)_3PtO . H_2O$ hydrate of oxyd of platinum.
$P(C_2H_5O)_3PtO . N_2O_3$ nitrate of oxyd " "
$P(C_2H_5O)_3 . PtN_2H_4 . 2HCl$ chlorhydrate of trioxethyl-phospho-platin-diamin.
$P_2Cl_6 . PtCl_2$ chlorid of hexachloro-diphosphoplatinum.
$P_2(HO)_6 . PtCl_2$ diphospho-chloroplatinous acid.
$P_2(AgO)_6 . PtCl_2$ silver salt.
$P_2(C_2H_5O)_6 . PtCl_2$ chlorid of hexoxethyl-diphospho platinum.
$P_2(C_2H_5O)_6 . PtN_2H_4 . 2HCl$ " " " diamine.
$P_2(C_2H_5O)_3Cl_3PtCl_2$ chlorid of trichloro-trioxethyl-diphospho platinum.
$P_2(C_2H_5O)_3(HO)_3PtCl_2$ diphospho-trioxethyl-chloro-platinous acid.

Schützenberger points out the analogy between these compounds and the ammonia-platinum bases. In a third note he gives a method of isolating the radicals contained in the above described compounds. By treating the alcoholic solutions of the two chlorids $P(C_2H_5O)_3PtCl_2$, and $P_2(C_2H_5O)_6PtCl_2$, with zinc, viscid black masses are obtained, having respectively the formulas $P(C_2H_5O)_3Pt$ and $P_2(C_2H_5O)_6Pt$, and combining directly with chlorine to reproduce the original chlorids.—*Comptes Rendus*, lxx, 1287, 1414; lxxi, 69. W. G.

3. *On new derivatives of triethylphosphine.*—Cahours and Gal have studied the product of the action of platinic chlorid upon triethylphosphine, and have obtained a compound having the formula,* $P(C_4H_5)_3PtCl$, which they regard as the equivalent of the green salt of Magnus in the ammonia series. This body crystallizes from its solution in ether in voluminous transparent prisms of an amber yellow color. An alcoholic solution of this salt heated to 100° for several hours in sealed tubes, yields crystals of an iso-

* C=6 O=8 Pt=98·7.

meric white salt. When the yellow salt is boiled with water and triethylphosphine, a colorless salt is formed, which has the formula, $[P(C_4H_5)_3]$ PtCl and which the authors consider to be the phosphorous analogue of the salt of Reiset. It loses an atom of triethylphosphine when long kept, and leaves the white modification of the chlorid first described. With argentic oxyd and water it yields a strongly alkaline liquid; with the chlorids of gold and platinum well defined salts. Palladium forms a compound similar to the first described salt of platinum, and also forming amber yellow prisms. This does not, however, unite with another atom of triethylphosphine. When triethylphosphine is added to a solution of auric chlorid, $AuCl_3$, in alcohol a colorless crystalline salt is obtained which has the formula, $P(C_4H_5)_3AuCl$. Compounds of an exactly analogous composition were obtained with the chlorid of platinum, palladium and gold, and triethylarsine.—*Comptes Rendus*, lxx, 1380, lxxi, 208. W. G.

4. *On silico-propionic acid.*—By the simultaneous action of zinc-ethyl and sodium upon ethyl-silicic monochlorhydrine, $SiCl(OC_2H_5)_3$, Friedel and Ladenburg have obtained a liquid boiling at 158°·5 C. and having the formula, $Si(C_2H_5)(OC_2H_5)_3$, which they term tribasic silico-propionic ether. A concentrated solution of caustic potash does not set free in this compound the silicon in the form of silicic oxyd SiO_2, but gives a product having the formula, $SiC_2H_5O_2H$, which however cannot be obtained in this way in a state of purity. By heating silico propionic ether at 180° C. in a closed tube with chlorid of acetyl, the authors obtained a mixture of acetic ether and a body having the formula $SiC_2H_5Cl_3$. By treating with water the part of this liquid which boils at 90°–110°, chlorhydric acid and a white gelatinous body are formed; this last is the hydrate of silico-propionic acid. When dried at 100° the acid forms a white amorphous powder greatly resembling silicic oxyd, but easily distinguished from it by its combustibility. When heated it burns like tinder, disengaging combustible gases. The acid is insoluble in water, but dissolves in hot concentrated caustic potash, and is not precipitated from this solution by HCl, but only by NH_4Cl, like SiO_2, the residue after evaporation being unchanged silico-propionic acid. The new substance appears therefore to be a weak acid analogous to silicic acid, and presents the first known case of a silicic acid containing carbon. Its formula shows that it contains the group, SiO_2H, which may be termed silicoxyl, and which is the analogue of carboxyl, CO_2H. It is easy to see also that it forms one term of a group of homologous acids.—*Comptes Rendus*, lxx, 1407. W. G.

5. *On normal amylic alcohol.*—Lieben and Rossi have succeeded in obtaining synthetically the normal amylic alcohol, which bears the same relation to the alcohol already known which normal butylic alcohol bears to that obtained by fermentation. Normal cyanid of butyl yields normal valeric acid, which greatly resembles the acid already known, but which has an odor more closely resembling that of butyric acid. It boils at 184°–185° at 736^{mm}. When normal calcic valerate is mixed with normal calcic formate,

and the mixture is distilled in small portions at a time, valeric aldehyd is obtained, boiling at 102° C. This aldehyd, by the action of nascent hydrogen, yields the normal amylic alcohol. The alcohol much resembles that obtained by fermentation, but has a higher boiling point, 137° C. under a pressure of 740^{mm}. The authors have prepared from it the chlorid, bromid, iodid and acetate of amyl, all of which possess higher boiling points than the corresponding ordinary amylic ethers. The constitution of the normal alcohol must be expressed by the formula,

$$\begin{matrix} CH_3 \\ CH_2 \\ CH_2 \\ CH_2 \\ CH_2.OH \end{matrix} = C\begin{cases} CH_2.CH_2.CH_2.CH_3 \\ H \\ H \\ \Theta H \end{cases}$$

while common amylic alcohol has probably the formula attributed to it by Erlenmeyer,

$$\begin{matrix} H_3C\,CH_3 \\ \diagdown\!\diagup \\ CH \\ CH_2 \\ CH_2.OH \end{matrix} = C\begin{cases} CH_2.CH(CH_3)_2 \\ H \\ H \\ OH \end{cases}$$

The authors promise a detailed description of the normal valeric acid and its salts.— *Comptes Rendus*, lxxi, 369. W. G.

6. *Transformation of the fatty acids into the corresponding alcohols.*—Saytzeff has given a method of passing from the fatty acids to the corresponding alcohols, which possesses much interest. An amalgam of sodium of 3 per cent is to be introduced into a flask, and a mixture of one molecule of the chlorid and two of the acid introduced, the mixture being cooled. After 12 hours water is added, the liquid distilled, and the distillate saturated with potassic carbonate. The product separated is the ether formed by the fatty acid with the corresponding alcohol. This may then be saponified by potash. In this manner the author prepared propylic and butylic alcohols.—*Bull. de la Société Chimique*, xiii, p. 51. W. G.

II. GEOLOGY AND MINERALOGY.

1. *Laurentian Rocks of Nova Scotia;* by Rev. Dr. Honeyman, F.G.S., &c.—In this communication I propose to make a few observations bearing upon remarks made by Dr. Hunt on the above subject, in the July number of the American Journal of Science.

While I was engaged in the service of the Canadian Survey, ascertaining the extent of the distribution of the Upper and Middle Silurian rocks of Arisaig, I unexpectedly came upon a band of crystalline rocks, of considerable thickness and of great lithological variety. There was a succession of different kinds of diorites and hornblendic rocks, traversed with veins of quartz and many granular limestones. These extended about two miles along the shore, stretching at the same time both into the sea and

land. After these came a thick band of ophites and ophio-calcites. Succeeding these were bands of diorites and hornblendic slates, with quartz veins containing mica in crystals, and then white feldspar rock of considerable thickness with green veins, and a thick band of rose-colored feldspar. I was convinced that I had discovered a series of Laurentian rocks. In geological position it is unquestionably inferior to the Arisaig series, which ranges from the Medina and Oneida through the Clinton, Niagara (?), Lower Helderberg, and possibly the Devonian, into the Lower Carboniferous. There appeared to me to be a sufficient lithological resemblance, between these rocks and those of Canada and Newfoundland Laurentian, to warrant the conclusion. Dr. Hunt gives me the credit of having profited by his suggestions, and the descriptions of Laurentian rocks given by the Canadian Survey, and assures the readers of the Journal, that these first brightened my ideas in regard to the geological age of the rocks in question. Dr. Hunt, however, knows right well that I had other and much better means of acquiring the requisite knowledge. I spent the greater part of six months in the Paris Exhibition, in the immediate vicinity of the Canadian Survey collection of rock specimens, from the Laurentian rocks of Canada and Newfoundland, and those who know me know that I am not in the habit of allowing such good opportunities to pass unimproved. After Mr. Richardson had finished the arrangement of these rocks in the Canadian department of the Exhibition, when there was no geologist in the Canadian and Newfoundland courts, my duties as Executive Commissioner in the Nova Scotia department requiring my constant attendance, I had frequent occasion to direct the attention of English and Continental geologists to the rock specimens referred to; so that I was not altogether ignorant of their character and appearance.

Thus schooled I considered the rocks discovered to be *Laurentian*. The first to whom I communicated the discovery was H. R. Hill, Esq., High Sheriff of Antigonish, who had often accompanied me in my Arisaig trips, and whose knowledge of the Silurian rocks of Arisaig and the places where fossils occur is somewhat intimate. To him I declared positively that I had found the Laurentian rocks in the neighborhood of our old geological field; at the same time I gave him specimens of these rocks. Shortly after I left the field on account of the inclemency of the weather. On my way home I had to pass through New Glasgow; I called upon Sir W. Logan at his hotel and showed him specimens of the ophite and ophiocalcite. He appeared to hail them as an important discovery, declared them to be of Quebec age, and recommended a search for chromic iron in the locality. When Prof. Hind inspected the specimens which I had in the Museum, he appeared then to regard them as Sir W. Logan had done, and in his report on the Waverly Gold Fields he mentions the discovery as a discovery of Quebec rocks. In the month of December following, I received a letter from Sir W. Logan, addressed to the care of W. A. Hendry, Esq., Deputy Commissioner of Nova Scotia Crown Lands, a gentleman who has had considerable field experience as

a geologist, to whom I communicated the contents of the letter. In this letter Sir W. Logan says "after all it is not serpentine you have discovered. Dr. Hunt says it is agalmatolite or dysyntribite and it proves nothing; the rocks may be of Upper Silurian or Devonian age." I was not prepared to maintain that the ophite was not agalmatolite or dysyntribite; simple test proved that the ophiocalcite was undoubtedly a calcite. I could not for a moment admit that the strata in question were Upper Silurian or Devonian. Any one knowing the locality could not fail to be convinced that they were underlying the Upper and Middle Silurian of Arisaig, and therefore might be Lower Silurian, Huronian, or Laurentian, but certainly nothing more recent I had a specimen of the ophiocalcite—ophiocalcite it is now acknowledged to be—polished on two opposite sides by Mr. Wesley, marble worker. In returning it to me he said he had partly polished it by means of acid. It turned out to be a very beautiful and peculiar specimen. I examined its surface by a combination of lenses having a considerable magnifying power, and observed what appeared to me to be structure. I had often examined the magnificent specimens of Eozonal serpentine which were exhibited in the Canadian department of the Paris Exhibition of 1867, which were afterward deposited in the Ecole des Mines of Paris. I also had received from a Bohemian geologist in Paris a specimen of ophiocalcite with *Eozoon Bohemicum.* I compared this with the Nova Scotian specimen; I became impressed with the conviction that if the Bohemian was Eozonal so was the Nova Scotian, and that this was another evidence of the Laurentian age of the Nova Scotian rocks. In my simplicity and ignorance of facts advanced in the Eozonal controversy, I did not know that serpentines of *all ages* might contain *species* of the genus Eozoon. I pointed out the peculiar structure of the Nova Scotian specimen and its resemblance to that of the Bohemian to Mr. Hendry, already referred to. He expressed doubts on the matter of structure in both, as an unbeliever in the Eozoon might do. About the same time Prof. Lawson of Dalhousie College and University, Halifax, came to the Museum and asked if I had a specimen of the *Eozoon Canadense;* I told him that I had not, but that I had a specimen of the *Eozoon Bohemicum* and what might be called the *Eozoon Nova-Scoticum.* He examined them, but replied that he wanted to see the real *Eozoon Canadense.* Mr. Hendry confirms the above statements as far as he is referred to, in the presence of Dr. How of Kings College and University, Windsor, to whose friendship and kindness I am indebted for the loan of a copy of the number of the American Journal of Science, containing Dr. Hunt's article already referred to.

In the spring of 1869, I visited Montreal and took with me the polished specimen of ophiocalcite. I gave it to Dr. Hunt in the lapidary's workshop in Gabriel street. A day or two after, when I was engaged in the same place talking with Sir W. Logan, Dr. Hunt entered with the specimen referred to in his article, and stated to Sir W. Logan in my presence to the effect that Dr. Honeyman had discovered a very interesting series of rocks; that

the specimens tempted him to go to see them, and that he believed them to be the Laurentian. Sir W. objected, saying that the specimen was not serpentine. Dr. Hunt assured him that it was ophiocalcite; that he had examined it; that the white effervesced with acid. He also named some of the constituents; I do not recollect what they were. Sir W. and Dr. Hunt left. I heard nothing more about the matter until my very kind host, Dr. Dawson, on returning from a visit to Gabriel street, told me, in his library, that Dr. Hunt had said to him that certain rocks that I had discovered were of Laurentian age; but that he had advised him to consider them first of all Devonian. All that I replied was, I heard him say so. For certain private reasons I expressed no opinion on the subject. When I returned to Halifax I arranged my representative collection of Nova Scotia rocks in the Provincial Museum under my charge, thus: Arisaig ophite, ophiocalcite and porphyritic diorite, at the bottom of the series; granites, new gneissoid rocks of Prof. Hind, next; andalusite rock and slates next; and thus they had remained until the present time. Many a time have I pointed out these rocks to visitors who take an interest in such matters, and told them the lowest ones were Laurentian. I have certainly not been silent, as Dr. Hunt informs the readers of the American Journal of Science, although the sound of my voice may not have reached Gabriel street, Montreal. I could name many witnesses to attest this.

I would now observe that, from one cause or other, I had never met with Prof. Hind since the time already referred to, until a day or two after he had read a paper on his discovery of the Laurentian rocks, before the Nova Scotia Institute of Natural Science. He came to meet Dr. Lawson in the Museum. He commenced discussing the subject of his discovery. I then turned his attention to my collection of rocks; showed him the Laurentian of Arisaig, and gave him a detailed account of the opinions expressed in regard to them. He informed me that he had announced his discovery of the Laurentian gneissoid in Sherbrooke and elsewhere, to Sir Roderick Murchison, Sir W. Logan, &c. Not long after a letter was inserted in the Halifax Morning Chronicle, addressed by Prof. Hind to the Hon. Robert Robertson, M.E.C., Commissioner of Mines and Public Works, dated Windsor, 10th Feb., 1870, from which I give the following extracts: "Under date Montreal, Feb. 3, 1870, Dr. Hunt informs me that recent microscopic examination of some of the specimens sent by Dr. Honeyman, has revealed *well-defined* '*Eozoon Canadense*.'" "This," continued Dr. Hunt, "must, I conceive, be *conclusive evidence of their Laurentian age*." "Dr. Dawson, in a separate communication, confirms the identification of the Eozoon."

I find, however, that in the discussion that followed the reading of the paper, "On the Laurentian rocks of Arisaig, N. S.," addressed to the Geological Society of London, Dr. Dawson remarked that the Arisaig Eozoon was different from that of Canada; that the *Eozoon Bohemicum* belonged to a formation more recent than the Laurentian, and that the Arisaig Eozoon did not prove the

rocks to be Laurentian. *Vide* printed abstract of the proceedings of the Geological Society of London.

I do not regard the discovery of Eozoon as the only evidence of the Laurentian age of the Arisaig rocks. I consider that *relative* position, in connection with *lithological* character, must have something to do with the determination of their age. Besides, is it not possible that, as there appear to be *new* different species of Eozoon belonging to different countries and different ages, that there may be different species in different countries, but still of the same age, and still that the Nova Scotian species is still Laurentian.

Another extract from Prof. Hind's letter: Dr. Hunt says, in his letter already referred to, "A line from the Laurentian of Malignant Cove (Arisaig) to that in Newfoundland, will pass through Cape Breton, and we can now look for limestone and Eozoon there." Is it not possible that the thought may have occurred to some one else besides Dr. Hunt? Perhaps Dr. Hunt has forgotten something that happened during the Exhibition in Paris of 1867. He came into the Nova Scotian Court, accompanied by Prof. Lesley of Philadelphia; he found among the polished specimens of marble a green one which arrested his attention; he asked where it came from, and was told that it came from Cape Breton, (having been furnished by Mr. Hendry already referred to.) This specimen also attracted the attention of Prof. Wyville Thompson, an Eozoonal unbeliever, who was a member of the International Jury; he said that it had Eozoonal structure; he wished to have it and got it; he took it to London where it made no small stir among the believers in the Eozoon, and gave occasion for a little ingenious explanation on the part of Dr. Hunt; so I was informed.

Another extract from Prof. Hind's letter: "It is thus, that Dr. Honeyman's opinions have been beautifully verified, but it would have greatly enhanced the gratification which Dr. Honeyman must feel if the announcement had been made a year and a half ago."

As might be expected this observation caused some excitement in Gabriel street. In order to rebut the above implied charge, doubtless, Dr. Hunt finds fault with me for not expressing my views, relative to the age of the rocks in question, in my official report. I answer, I studiously avoided expressing any opinion on controverted points in my report, and made it a simple record of facts. As far as I recollect, I mentioned the discovery of the rocks *simpliciter*. It appears that my alleged silence is considered by Dr. Hunt as "*simply incomprehensible.*" I think I can bring the matter to the level of the comprehension of even a less acute person than Dr. Hunt. I have already shown that the term *silence*, if meant literally, is not a proper term to use in the case, as I have not been *silent* on the subject. If by silence he means that I had not addressed a communication on the subject to any scientific Society or Journal, until the publication of Prof. Hind's letter, Dr. Hunt has hit the mark. My defense is, I had not been able on account of the inclemency of the weather and lateness of the season, to examine the rocks in a manner so thorough and satisfactory as was desirable; I did not think there was any great hurry in the

matter, as I did anticipate the *great discoveries* of my excellent friend Prof. Hind. I have not yet had the desired opportunity, and yet I have had to write on the subject: first, to the Geological Society of London at the request of Prof. Hind; second, to the American Journal of Science, at the instigation of Dr. Hunt.

2. *Descriptions of Fossils collected by the U. S. Geological Survey under the charge of Clarence King, Esq.* (Proc. Acad. Nat. Sci., Philad., No. 1, 1870).—Mr. MEEK prefaces his numerous descriptions of new species by the following observations, addressed to Prof. Joseph Leidy.

I send herewith, to be presented for publication in the Proceedings of the Academy, descriptions of a few of the fossils brought in by the United States Geological Survey under the direction of Clarence King, Esq. You will please state, in presenting the paper, that the Trilobites described in it from Eastern Nevada, are decidedly Primordial types, and, so far as I know, the first fossils of that age yet brought in from any locality west of the Black Hills. Mr. King's collections also establish the fact that the rich silver mines of the White Pine district occur in Devonian rocks, though the Carboniferous is also well developed there. The Devonian beds of that district yet known by their fossils, seem mainly to belong to the upper part of the system. Mr. King, however, has a few fossils from Pinon Station, Central Nevada, that appear to belong to the horizon of the Upper Helderberg limestone of the New York series.

The Tertiary fossils described in this paper, from the region of Hot Spring Mountains, Idaho, came from an extensive and interesting fresh-water Lacustrine deposit, and are all distinct specifically, and some generically, from all the other Tertiary fossils yet brought from the far west. Two of the species belong to the existing California genus Carinifex, or some closely allied group, while another beautifully sculptured species was thought, by Mr. Tryon, to whom I sent a specimen of it, to be possibly a true *Melania*, and allied to existing Asiatic forms.

It is an interesting fact, that among all of our fresh-water Tertiary shells from this distant internal part of the Continent, neither the beaks of the bivalves, nor the apices of the spire in the univalves, is ever in the slightest degree eroded; even the most delicate markings on these parts being perfectly preserved, if not broken by some accident. From this fact it may be inferred that the waters of the lakes and streams of this region, during the Tertiary epoch, were more or less alkaline, as is the case with many of those there at the present day.

These descriptions, as well as others that I expect to send you soon, are merely preliminary and will be re-written, and presented with full illustrations, now in course of preparation, in Mr. King's report of his survey.

3. *Discovery of a Mastodon.*—On Friday last, Mr. Fletcher Correll, a farmer residing one and one-half miles southeast of Illiopolis, in this county, was digging a well upon his place, when, at the depth of about four feet from the surface, he struck a hard sub-

stance, at first supposed to be a stone or piece of wood imbedded in the earth; but upon digging farther, it was discovered to be the remains of a mastodon.

The bones were in a fair state of preservation, and exhibited a dark, spongy, porous appearance. One of the tusks, which was broken in removing it from its long resting place, proved to be, when measured, nearly ten feet in length, and twenty nine inches in circumference three feet from the lower end. The other tusk, and the main portion of the skeleton, are now being lifted from the earth, and will probably be added to the collection of fossils now being made by our state Geologist, Professor Worthen.

The part of this huge creature which was exhibited in our office in the presence of Professor Worthen, was a piece of the lower left jaw, about two feet in length, and at least the same in circumference. It contained, in a fine state of preservation, one of the great jaw-teeth.—*Daily State Journal, of Springfield, Illinois, Sept. 7. Received from C. L. Conkling.*

4. *Description of the Cavern of Bruniquel, and its organic Contents;* by Professor Owen. Part I, Human and Equine Remains. 55 pp. 4to, with six plates. (Phil. Trans., 1870; communicated to the Royal Soc., June, 1864).—Professor Owen, after describing the human remains, draws from them the following conclusions:

"They exemplify the distinctive characteristics of the human genus and species, as decidedly as do the corresponding parts of the present races; they show most affinity with the oldest Celtic types, the cranium being oval, and rather dolicho- than brachy-cephalic in its general proportions; the cranial capacity or brain corresponds with that of uneducated Europeans of Celtic origin, and exceeds that of the average Australian aboriginals."

Some of the bones accompanying the human remains—those of the deer especially—are covered with drawings, representing the heads of horses and showing much artistic skill; and from these draughtsmen of the cave-dwelling people, we thus learn that the horse of the era, *Equus spelæus*, had short pointed ears, and that the stallions had beard-like hairs; and from the antler of a reindeer, found in another cavern, that of La Madelaine, in Dordogne, we have the additional fact that the tails of these early horses were short and furnished with long hairs to their base, having "cauda undique setosa," instead of "cauda extremitate setosa," a fact repeated seven times on the antler. The horse is of the same species that occurs in certain quaternary beds in France, for example, those near the Tour de Juvillac, Püy-de-Dome, and belongs to the restricted genus *Equus*, and not to that including the zebra and ass (*Asinus*). Prof. Owen concludes with the following paragraph:

"No satisfactory evidence of an aboriginal feral *Equus caballus* has yet been obtained by the Naturalist. No specimen of such exists in any Museum. The doubts expressed by Forster and Pallas as to the alleged wild horses of the Ukraine, viz: that they might be descendants from strayed domestic horses, have not yet been cleared up. I believe the illustrations contained in the pres-

ent paper to be the best, if not sole, evidences of the wild originals of some of our domesticated breeds. Like the alleged wild horses of Prussia, those of Aquitaine, in the time of the flint-armed hunters and cave-dwellers, were doubtless "shy and difficult of capture, but very good venison."

The above memoir is followed by another, under the same cover, entitled *Supplementary Remains of Equines from Central and South America referable to* Equus conversidens *Ow.*, Equus tau *Ow.*, and Equus arcidens *Ow.* In this paper Professor Owen, besides discussing the characters of the different kinds of fossil horse in the regions referred to, treats of their relations to the extinct genera *Macrauchenia*, *Nesodon*, and *Toxodon*, and illustrates the subject with numerous figures on six lithographic plates. He observes that according to the facts made out, the most ancient Equines of the South American continent present modifications of the upper molars, and more especially of the lower molars, which supply an additional link between the Equines with other perissodactyle Ungulates, and the group including *Toxodon* and the other genera above mentioned.

5. *The North American Lakes as Chronometers of Post-glacial time.*—Dr. E. Andrews, the author of this paper, noticed at p. 264 of this volume, states, in a recent letter, that his method of calculation eliminates the error alluded to in the notice, due to the wearing away of the sands. He says that in the calculation, "the first element is the age of the lower beach, which is obtained by ascertaining the age of its cotemporary terrace of erosion; this settles the approximate age of the lower beach without liability to error from attrition of sand. The age of the upper beaches is deduced by comparison of their relative masses with the lower. Now as the attrition was going on at about the same rate at all ages, the relative loss by attrition would be strictly proportioned in all the beaches, and then so far as time is concerned would be unchanged."

6. *Descriptions of new Fossil Shells of the Upper Amazon;* by T. A. Conrad, (Am. J. of Conch., Oct. 10, 1870).—Mr. Conrad describes several species of shells from the deposit at Pebas, on the Amazon, visited by Prof. Orton, (see p. 294 of this volume). They include a number of species of *Pachydon*, Gabb, and a bivalve near *Mulleria*, besides gasteropods of the genera *Isæa* Conrad, *Hemisinus*, *Dyris* Conrad, *Neritina*, *Bulimus*. The Bulimus is the only land shell in the collection. The species "may have lived either in fresh or brackish water, but are certainly not of marine origin." They appear to be all extinct, and if so, they "cannot be later than the Tertiary."

7. *Restoration of a Flying Dragon* (Dimorphodon macronyx *O.*) *from fossil remains in the Lias-cliffs of Dorsetshire;* by Prof. Owen, F.R.S. (Pages 463–502 of Pal. Soc. volume for 1869). With two plates. London, 1870.—Prof. Owen's memoir contains a full and detailed account of this remarkable Pterosaur, and illustrates it by admirable lithographs. He does not agree with Mr. H. G. Seeley with regard to the relations of Pterosaurs to Birds.

8. *Remarks on Prof. Owen's Monograph on Dimorphodon;* by H. G. SEELEY, F.G.S., (from the Ann. Mag. Nat. Hist., Aug., 1870. 24 pp.)—Mr. Seeley in this paper presents his objections to the views of Prof. Owen on the Dimorphodon.

9. *Geological Charts.*—The second edition of von Dechen's general geological chart of Germany, France and England and the adjoining countries, was published at Berlin in 1869, and a geological chart of Germany by the same able author in 1870.

The Royal "Comitato Geologico," of Italy have announced that they will publish, the coming year, a small geological map of Italy.

10. *Carte Géologique du versant Occidental de L'Oural;* by VALÉRIEN DE MÖLLER. 1869. Scale $\frac{1}{840,000}$. A beautiful colored geological chart of the Western or European slope of the Urals.

11. *Second Annual Report upon the Geology and Mineralogy of the State of New Hampshire;* by C. H. HITCHCOCK, Prof. Geol. and Min. in Dartmouth College. 38 pp. 8vo. Manchester, N. H. 1870.

III. BOTANY AND ZOOLOGY.

1. *Martius, Flora Brasiliensis:* fasc. xlix, *Cyatheaceæ et Polypodiaceæ.* Exposuit J. G. BAKER.—The other suborders of Filices were worked up by J. W. Sturm, and were published in 1859. To illustrate the different ways in which these two fern-authors regard species, one may look at the genus *Aneimia*, of which Mr. Baker in the "Synopsis Filicum" recognizes but twenty-six species for the whole world, while Sturm describes not less than thirty-one species peculiar to Brazil, besides thirteen common to Brazil and some other countries, and five more not yet reported from Brazil, but likely to be found there. Mr. Baker seems to diminish the number of species at every step; many that were recognized in the "Synopsis" being reduced to synonyms in the work now under consideration. For instance, *Lomaria L'Herminieri* and *L. lanceolata*, placed in different groups in the Synopsis, are now made into one species. So, too, *Blechnum Fendleri* is joined to *B. longifolium*, and *Polypodium* (*Phegopteris*) *rigidum* to *Aspidium aculeatum*. *Gymnogramme calomelanos* swallows up *G. tartarea*, having long ago digested *G. chrysophylla*, *Massoni*, *Peruviana*, &c., and many other such instances might easily be pointed out. But all this reduction seems to have been done only after patient study over many specimens, and it is gratifying to see this emphatic protest against the flood of new species with which we are deluged by continental authors.* The author's industry and re-

* The publication in Linnæa by Dr. Kuhn of manuscript names left by the late Dr. Mettenius, many of which the maturer judgment of the latter would doubtless have withheld, gives to Botany several hundred "new species;"—a few examples of them from one genus, by no means a large one, are subjoined.—*Pellæa intermedia*, founded on Mr. Wright's New Mexican No. 825, is said to be "perhaps a hybrid between *P. sagittata* and *P. flexuosa*," forms which in all probability both belong to *P. cordata*; *Pellæa myrtillifolia*, is the Chilian form of *P. andromedæfolia*; *Pellæa microphylla* is the new name for the New Mexican specimens of *P. pulchella*, and *Pellæa glabella* rests upon St. Louis and Wisconsin specimens of *P. atropurpurea*, with an unusually glabrous stipe and rhachis.

search have unearthed a few long buried and forgotten names for well known ferns:—*Pteris geraniæfolia* Raddi (1825) was found by Dr. Kuhn to be the same as *Pteris Pohliana* of Presl. (1822), and now we must go back twelve years more to *Pteris concolor* Langsdorff and Fisher (1810). Can any one find a yet more ancient name for it? It is gratifying to see, in a note, that the African *Cheilanthes Kirkii* is now confessed to be probably an adiantopsoid variety of this same species, the only difference being in the interrupted instead of continuous involucre.

Synonymy given by Mr. Baker is ample without being profuse: the characters are well drawn up, and the descriptions clear. There are several pages of letter press devoted to the geographical distribution of Brazilian Cytheaceæ and Polypodiaceæ. If the orders elaborated by Dr. Sturm could have been included in the notes and tables relating to geographical distribution, it would have been very acceptable to Pteridologists, but, as Mr. Baker remarks, the former suborders were written under such a very different theory of species that they could not be joined with the present ones in any kind of synoptical table. There are thirty-eight plates of Nature-printed fragments of Fern-fronds, borrowed from Ettingshausen, and thirty-two more cut on stone, from drawings by Fitch, Hipsley and Kunze, with a single one done under the eye of Dr. Eichler at Munich. D. C. E.

Oxford Botanists is the theme of a discourse pronounced by Prof. Lawson,—the successor in the professorial chair of the worthies he commemorates,—at the recent Congress held at Oxford by the Royal Horticultural Society. The history begins with the foundation of the Botanic Garden by Henry Earl of Danby, in 1632, and with the installation of *Jacob Bobart*, the first curator. It gives a biography of *Dr. Morison* the first professor, appointed in 1669; of his successor, *Jacob Bobart*, the second; of *Dr. Sherard*, or "Sherwood" who re-endowed the chair, and of *Dillenius* whose appointment he provided for in his will; of *Dr. Humphrey Sibthorp*, the next incumbent, and his distinguished son, *Dr. John Sibthorp*, who succeeded his father in 1784, and died at the age of 38 in 1796, bequeathing new endowments and the foundation of the associated chair of Rural Economy, after providing for the publication of the sumptuous *Flora Græca*. His successor *Dr. Williams* is mentioned; and an interesting notice of the late incumbent and patron, *Dr. Daubeny*, brings down the history to the present time.

Dr. Hasskarl, who has long been studying the *Commelynaceæ*, has just issued a monograph of the Indian species, especially of the Archipelago, with occasional genera and species of other parts of the world; an octavo of 182 pages, published at Vienna. Even in this partial representation of the order over forty genera are admitted! Some valuable criticisms of Dr. Hasskarl's work in this order will be found in a paper by Mr. Clarke, mentioned below.

The Journal of the Linnean Society, Nos. 54, 55, issued together in September of this year. The first and the most important, as well as the longest, article is

A Revision of the Genera and Species of Herbaceous Capsular Gamophyllous Liliaceæ, by J. G. Baker, F.L.S., &c. The adjectives here employed distinctly limit the portions of the great Liliaceous family, to which Mr. Baker here devotes himself. That he intends to go through the order in this way seems probable, and it is desirable. In treating incidentally of the limitation of *Liliaceæ*, he refers without qualification, to "characters which are universally regarded as of ordinal value, in the extrorse anthers, separated styles and septicidally dehiscent capsules of *Colchicaceæ*," apparently under the impression that these characters and their converse, run harmoniously parallel throughout. The exceptions, however, which have already been noted and placed upon record, and the way in which some of these opposed characters blend, may upon consideration show that so complete a separation is hardly practicable; and indeed the attempt has been abandoned by at least three botanists already, two of them authors of British Floras, and of whose views Mr. Baker would be likely to be particularly well-informed. *Helonias* and *Chamælirium*, on one hand, are loculicidal in dehiscence, while some near Japanese relatives of Helonias have united styles, and *Tofieldia* has introrse anthers. Then the two elements which constitute extrorsion or introrsion of the anthers may not coincide. The anthers of *Uvularia*, being adnate, are plainly extrorse, yet the lines of dehiscence are almost exactly lateral. If, then, we regard the fixation rather than the lines of dehiscence as the more essential, *Lilium* itself comes into the category of extrorse anthers (as first noticed, long ago, by Dr. Chapman), for its anthers are extrorsely attached although slightly introrsely dehiscent. *Medeola* is in the same case, while *Trillium* is nearly as in *Uvularia*. Mr. Baker brings the plants he deals with under 27 genera, including our *Hesperocallis*, which had escaped notice when the Clavis was printed. As to this very rare and little-known plant, he expresses a natural doubt as to its generic distinctness from *Hemerocallis*, which inhabits a widely distant part of the world. We are now able to state that, like *Hemerocallis*, the blossoms open in the morning and close at night. This is on the authority of Mr. Schott, who proves to have been the first discoverer of the plant. Moreover, which is of more consequence, he collected and has supplied us with the "bulb." It really is a *tunicated bulb;* and so the genus falls out of the tribe *Hemerocallideæ*, and seemingly into Mr. Baker's *Odontostemoneæ*, for which we would like to have Mr. Baker substitute the name of *Hesperocallideæ*. Our further interest in this revision, as regards N. American Botany, centers in *Brodiæa* and its allies. In this genus he (as well as Dr. Hooker, in Bot. Mag.) retains the remarkable *Brevoortia* of Mr. Wood, which, it appears, was first collected by the late Mr. Lobb; and he follows up the idea which this species suggested by reducing Torrey's *Stropholirion* likewise to *Brodiæa*. On the other hand he departs from Bentham's view, and refers all the hexandrous species, and we suppose justly, to *Milla*, which swallows up *Triteleia*, *Calliprora*, and even *Hesperoscordum*. The

latter name is accidentally printed *Hesperocordium* in the conspectus; and further on, *Hesperoscordium*. The latter is perhaps an intended (and a laudable) innovation, as it is hard to pronounce the original name, as must needs be, with the accent on the penult.

On the Commelynaceæ of Bengal, by C. B. Clarke; another paper in this fasciculus of no small importance for the correct understanding of the characters of these difficult plants.

Notes on some Algæ found in the North Atlantic Ocean, by Dr. Dickie. The results of the examination of the contents of a bottle which was filled in the midst of the Atlantic, lat. 12° N., long. 21° 40′ W. There Capt. Mitchell, on the 24th Nov. 1867, found a patch of sea, 12 or 14 miles across, closely studded with a green substance, some of which he gathered. It is thought to have been wafted there from the American side by the Gulf-stream and the African current. The principal interest to us is in the following statement:—

"The substance thus picked up by Capt. Mitchell was in excellent condition, and, besides three *Algæ*, contained numerous fragments, more or less decayed, of wood, both endogenous and exogenous; *seedling plants several inches long, all with a pair of cotyledons, roots, and terminal bud, quite fresh;* small fruits partially decayed, evidently one-seeded legumes; intermixed were various microscopical crustacea, and a common oceanic insect, one of the *Hydrodromideæ*, genus *Halobates;* on some of the pieces of drift wood were numerous elliptical ova, of a deep orange color, mixed with which was growing the smallest of the three Algæ now to be described."

Leaves do not absorb moisture from the atmosphere in the state of vapor, according to a series of elaborate experiments by Unger, and afterward by Duchartre,—some of which were published several years ago,—confirmed recently by those of Prillieux.

The latter reaches this conclusion upon ascertaining that when a leafy branch, flaccid from evaporation, is suspended in moist air and recovers its freshness—as every one knows it promptly does—it does not gain in weight, but even loses. The same occurs with leaves furnished with petiole, the cut end sealed with wax. The conclusion is that the moisture by which the foliage is freshened comes not from the surrounding air but from the lower part of the branch, or from the leaf-stalk, the upper and foliaceous parts gaining at the expense of the lower. To test this, let the experiment be tried with single leaves destitute of leaf-stalk. Duchartre made a portion of his experiments with epiphytes, both Orchids and *Bromeliaceæ*, and concluded that the same applied to the aerial roots as well as the foliage. There was no gain in weight except when liquid water reached the leaves, roots, or other portion of the surface, when absorption was promptly indicated by increase in weight: and it appeared that this absorption of liquid water hardly took place by the leaves unless these were immersed or kept long and thoroughly wet. This again corresponds with some older experiments which went to show that syringed foliage did

not absorb the liquid. As to aqueous vapor, there being free communication by the stomata between the air outside of the leaf and that within its intercellular spaces and passages, and the thin walls of the parenchyma-cells freely allowing vapor of water to pass out, it is very difficult to believe that it will not equally pass inward under appropriate conditions. Hence we cannot yet regard the question as concluded. The editor of the Gardeners' Chronicle (Sept. 17) pertinently suggests that "it does not appear to have been remembered that the density of aqueous vapor is very much less than that of dry air. If the latter be estimated at 1000, that of aqueous vapor is 625 only,—a circumstance that ought to be taken into consideration in such experiments." But if leaves continue so to absorb aqueous vapor, they ought to condense or appropriate it, along with carbonic acid gas, and so increase manifestly in weight.

An Elementary Course of Botany, Structural, Physiological and Systematic, by the late Prof. Henfrey, which was originally published (by Van Voorst) in 1857, is this year reproduced in a second edition, "revised and in part rewritten" by Dr. Maxwell T. Masters, F.R.S., &c.—The death of Prof. Henfrey, while still a young man, was a sad loss to physiological botany in Great Britain. As a vegetable anatomist he was becoming truly eminent. Our estimate of the value of this, his last work—on the whole very favorable—was recorded at the time in a somewhat extended review of it in the number of this Journal for November, 1857 (vol. xxiv, II, p. 434–440.)

The preparation of a second edition could hardly have been consigned to better hands than those of Dr. Masters, who has evinced special aptitude for morphological work, which was not Prof. Henfrey's *forte*. The alterations and additions by the present editor are not specially indicated: those which we have discovered are well adapted to bring the work up to the time, and to render it a valuable text-book. But we are bound to maintain that it might have been more thoroughly "revised and rewritten" to great advantage. It could hardly be expected that criticisms made a dozen years ago, and on this side of the Atlantic, would come under the notice of a recent editor, yet it is curious to observe that, out of a series of more than twenty misapprehensions or errors in detail (individually of small moment, no doubt), which were indicated in this Journal, chiefly in the morphological part, all but two (and one of these a grammatical slip) stand unaltered in the new edition. The sole alteration is in the statement which attributed axillary tendrils to the Vine. We do not include some criticisms of another order, relating to mere theoretical points, upon which views which have long prevailed are retained;—such, for instance, as that which unquestioningly regards the so-called radicle of the embryo as root, although in one part of the work its nature as hypocotyledonary stem is implied in a particular instance. That this part of the embryo represents the first joint of stem is a doctrine which seems to be ignored in England, but generally accepted out of it. A. G.

2. *Anatomisch-Systematische Beschreibung der Alcyonarien. Erste Abtheilung; die Pennatuliden, erste Hälfte*, von A. Kölliker. Quarto, with ten plates. Frankfort, 1870.—There are very few groups in the animal kingdom which have been more carefully studied, during several years past, than the Halcyonoid Polyps, or concerning which our information has more rapidly increased, both in respect to their structure and the number and variety of their forms. The Pennatulacea, especially, have been carefully studied and described by several able naturalists, and the number of species has been increased to an extent that is truly surprising, considering the very few species known a dozen years ago, and their comparative rarity. Numerous species and genera were made known in the works of Herklots, Bleeker, Richiardi, and others. But in this work Prof. Kölliker has undertaken a complete revision, both anatomical and descriptive, of the entire group, and in the part already published has added a great number of new and interesting forms. His work is of greater interest from the fact that he has been able to examine the collections in nearly all the principal museums of Europe, including the types of most previous writers.

This first part includes an anatomical description of the whole group, with an analytical table of the classification adopted, while most of the volume is devoted to very careful and elaborate anatomical descriptions of the genera: *Pteroeides*, *Godeffroyia* (nov.), *Sarcophyllum*, *Pennatula*, *Leioptilum*, *Ptilosarcus* and *Halisceptrum*, and their included species, of which a large proportion are admirably illustrated. In the genus *Pteroeides* 43 species are described, of which 29 are regarded as new, although a few of them are probably identical with some of those described in the nearly contemporary work of Richiardi, (we have not learned the precise date of either). Numerous varieties of some of the species are also named and carefully described. The polymorphic forms of the polyps in all the genera and species are thoroughly discussed

The following is the classification adopted:

First Tribe, Pennatuleæ; Second Tribe, Renillaceæ; Third Tribe, Veretillidæ.

Tribe, Pennatuleæ.

1st Family, Penniformes.

1st Subfamily, Pteroidinæ, including the genera *Pteroeides* Herkl., *Godeffroyia* Köll., *Sarcophyllum* Köll.

2nd Sub-family, Pennatulinæ, including *Pennatula* L., *Leioptillum* Verrill, *Ptilosarcus* Gray, *Halisceptrum* Herkl.

2nd Family, Virgularieæ, including *Virgularia* Lam., *Stylatula* Verrill, *Pavonaria* Köll., *Scytalium* Herkl., *Funiculina* Lam., *Halipteris* Köll. V.

3. *The External and Internal Parasites of Man and Domestic Animals*; by A. E. Verrill. 140 pp. 8vo, 85 wood-cuts. (From the Report of the Connecticut Board of Agriculture), Hartford, 1870.—The first part of this work is devoted to the external parasites and contains descriptions of the various species of fleas, lice,

ticks, bot-flies, bed-bugs, mites, and acari, often troublesome to our domestic animals, as well as to man himself, together with accounts of their habits, effects, and remedies. Several pages are also devoted to introductory remarks on the structure of insects and their classification.

The second part is devoted to the internal parasites, and contains a general account of the parasitic worms, and their classification, a series of lists of those found respectively in man, the dog, cat, sheep, cattle, horse, hog, and poultry; and detailed descriptions of the forms, anatomy, metamorphoses, habits, effects, and remedies of some of the most important species, many of which are illustrated by wood-cuts. Among those most fully discussed are the *Trichina spiralis;* the pork and beef tape-worms of man; the *Tænia cœnurus* or water-brain of sheep; the *T. echinococcus* or hydatids of man, sheep, etc.; the *Fasciola hepatica* or liver-fluke of sheep and cattle; the *Syngamus trachealis* or gape-worm of chickens, etc., and many others of importance.

4. *On the Eared Seals* (*Otariadæ*), *with detailed descriptions of the North Pacific species;* by J. A. Allen. *Together with an account of the Habits of the Northern Fur Seal* (*Callorhinus ursinus*); by Charles Bryant. 108 pp. 8vo, with three plates. From the Bulletin of the Museum of Comparative Zoology, vol. ii, no. 1. Cambridge, Mass., August, 1870.—This is a very interesting and important contribution to our knowledge of the seals of the Pacific and Southern oceans. It includes a résumé of the recent literature of the subject; a chapter on the affinities, distinctive characters, variations due to age and sex, etc., and a conspectus of the genera and species; detailed descriptions of *Eumetopias Stelleri* Peters, based on two perfect skins and two ligamentary skeletons; of *Zalophus Gillespii* Gill, based on two skulls: and of *Callorhinus ursinus* Gray, based on six skins and four complete and two incomplete skeletons, representing both sexes and the young and old. Mr. Bryant contributes a very full and interesting account of the habits of the last species, with descriptions of the islands that they inhabit, the modes of capture, etc. Much information, some of it new, is also given from various other sources, concerning the habits of this and the other species. The geographical distribution of the family is also discussed on page 42.*

The plates and several wood-cuts are devoted chiefly to the skulls and teeth. V.

5. *Record of American Entomology for the year* 1869. Edited by A. S. Packard, Jr., M.D. Naturalist's Book Agency, Salem, Mass.—This, like the Record for 1868, is a work that should be in the hands of every entomologist who hopes to keep up with the

* It is here stated that "the habitat of no species, so far as certainly known, quite reaches the tropics," but a skull of *Otaria jubata* in the Harvard Anatomical Museum labeled as from "Arica, Peru," is mentioned, with a doubt as to the correctness of the locality. I may add that in the Museum of Yale College there is a large adult skull of the same species, collected by Prof. F. H. Bradley much farther north, at Zorritos, Peru (about lat. 5° S.) where the marine fauna is eminently tropical and nearly identical with that of Panama.—A. E. V.

progress of the science. It contains references to the writings of fifty-two authors, with indications of the species referred to, including 335 described during the year as new, in American works.

6. *Antero-posterior Symmetry, with special reference to the Muscles of the Limbs;* by ELLIOTT COUES, A.M., M.D., Ph.D.—Dr. Coues takes up the idea, well illustrated by Dr. Jeffries Wyman, of antero-posterior polarity in the animal system, and points out the facts sustaining it in the Muscles of the Limbs. His valuable discussion of the subject runs through the several recent numbers of "The Medical Record," (published in New York), from June 1 to September 1; Article v, in the series, is contained in the last mentioned number.

7. *On the Hypothesis of Evolution;* by EDWARD D. COPE. (From Lippincott's Magazine, for July, Aug. and Sept., 1870.)—Prof. Cope has brought an extensive knowledge of science to bear on the question of evolution in animal life, and presents some original views. The subject is too large a one for discussion in this place.

IV. ASTRONOMY.

1. *Elements of the new planet Ate;* by Dr. C. H. F. PETERS. (Communicated by the author in a letter dated, Litchfield Observatory of Hamilton College, Clinton, N. Y., October 23d, 1870.—The following elements of the planet *Ate* (111) were derived from intervals of 18 days, viz: from observations of Aug. 14, Sept. 1 and 19:—

Epoch: 1870. Sept. 0·0. Berlin mean time.

$M_0 = 205°\ 17'\ 21''{\cdot}0$

$\pi = 122\ 53\ 7{\cdot}3$

$\Omega = 306\ 26\ 28{\cdot}4$ } Mean Equ. 1870·0.

$i = 5\ 1\ 21{\cdot}4$

$\varphi = 5\ 49\ 10{\cdot}6$

$\mu = 858''{\cdot}392$

$\log a = 0{\cdot}4108808.$

They represent an observation obtained last night still within a few seconds.

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *On the Chemistry of the Bessemer process.*—The following is an abstract of the paper on the Chemistry of the Bessemer process, read before the American Association for the Advancement of Science, at its Troy meeting, by Lieut. C. E. Dutton, U. S. A.

After some general statements concerning the chemical composition of crude pig iron, Lieutenant Dutton passed to discuss the theoretical changes possible when air is brought in contact with it in a melted state. According to his view, the silicon is first oxydized, then the phosphorus, next the manganese and sulphur, and lastly, the carbon. Chemically, there is nothing new in the Bes-

semer process; it may be said to be "the employment of entirely new mechanical methods and appliances for effecting old and familiar reactions." The process itself was then minutely described. Since the sulphur is but partially removed and the phosphorus not at all, the iron selected must be free from these substances. It must also contain at least two per cent of silicon. This iron is melted in a cupola furnace, run into the converter—a charge being 12,000 pounds—and the blast turned on. "The first change is in the oxydation of iron and silicon. The silicon becomes silicic acid and enough of the iron oxydizes to satisfy the affinities of the acid and does not decompose during the remainder of the blast. It is during this stage of the conversion that the remarkable heat of the conversion is developed. It will be remembered that when silicon oxydizes, it takes up three equivalents of oxygen. Carbon takes up only one in this process, becoming carbonic oxyd. It is a common error to suppose that any very great quantity of heat is generated by the combustion of the carbon,—that is, as compared with that derived from the silicon. * * * The heat generated by the silicon burning to silicic acid will be found by the application of the coefficients and formulæ of the mechanical theory of heat, to be from two and one-half to three times greater than that generated by the burning of the carbon to carbonic oxyd. Another circumstance of importance is that the silicic acid remains as a dense fluid in the converter, no part of its heat being lost, except such as is carried out of the converter by the atmospheric nitrogen; and none is rendered latent by converting it into vapor; while the carbon is vaporized, a physical change absorbing much heat, and the vapor thus formed is carried out of the converter at a very high temperature. Hence will be seen the necessity of employing irons containing high percentages of silicon. At least two per cent of this element is essential, any less quantity being insufficient to generate heat enough to keep the iron thoroughly liquid and fluent until the end of the casting process. It is often asserted that irons for Bessemer conversion must be 'gray irons,' as they are called; i. e., irons rich in carbon. Now, although this happens as a rule, to be true enough, it is apt to lead to misapprehension. The fact that Bessemer pig irons are carbonized varieties is an accident, and not an essential feature. What is essential is that it should contain a large quantity of silicon, and very little—indeed, the least possible—of sulphur, phosphorus, and manganese. Now, a pig-iron containing much silicon and no sulphur or manganese, is pretty sure to contain a high percentage of carbon, as all smelters are aware. This fact is a feature of the blast-furnace, and almost without exception. If an iron could be produced with much silicon, a little carbon, no phosphorus, it would I think, be not altogether unsuited to the Bessemer treatment. In a word, the quantity of carbon is approximately immaterial, except so far as it implies proper conditions with respect to other elements. The main element required is the silicon and not the carbon." Of phosphorus, "the arch-enemy of the iron-maker, but the very scourge and pestilence of

the steel-maker,"—fifteen-thousandths of one per cent ruining Bessemer metal past all remedy,—Lieut. Dutton said: "I have already ventured the opinion that phosphorus increases its affinity for iron with every increase of heat; at least relatively if not absolutely. The fact seems to be, absolutely. If we accept it, we can instantly explain what seem, otherwise, to be many anomalies and contradictions. It will explain to us why, in the great heat of the blast-furnace, it leaves every other combination and enters the iron; why, in the much lower heat of the puddling furnace it seems to waver between staying with the iron, or forming a new alliance with oxygen, ready to choose either, at the influence of any third substance which may affect the question; why in the Bessemer process it clings to the iron with a desperate tenacity which nothing seems able to resolve. These three facts, then, are all of them formidable: 1st. That a minute quantity of phosphorus is capable of working terrible injury, and that it is omnipresent throughout nature; 2d. That whatever quantities of it are charged into the blast-furnace, as fuel, flux, or ore, are almost wholly concentrated in the resulting pig-iron; and 3d. That no portion is eliminated in the Bessemer converter." The sudden change of the flame at the close of the conversion Lieut. Dutton thus explained: "When two combustibles are intermixed, like oxygen and hydrogen, or hydrocarbon gas, it is well known that the relative proportion of the two elements in the mixture influences the readiness with which they combine. Thus oxygen and hydrogen cannot combine explosively unless their proportions lie within certain definite and rather narrow limits. May not the same law hold good in the present case? It is certain, or nearly certain, that the iron either does not oxydize in the bath during the blow, except in quantity sufficient to furnish a base for the acids present; or, if it oxydizes beyond that, it is immediately reduced again, leaving little or no free oxyd of iron in the bath. But after the change of flame all this is reversed and iron oxydizes rapidly and freely, and remains undecomposed, while the residual traces of the other elements as suddenly cease to oxydize rapidly. I freely grant that in referring this back to what is supposed to be a conceded, but unexplained law, we are merely putting the question in another, a more general, and more abstract shape:—still it is, in a qualified sense, an explanation." The theory of the action of the spiegeleisen, run in after the blast is stopped is next discussed, and also the quality of the metal produced, which Lieut. D. calls a "cast wrought-iron." The paper closed with some comparisons of Bessemer with other metal, and a discussion of the uses for which Bessemer metal is most valuable. G. F. B.

2. *Earthquake of October 20th*, 1870.—On Thursday morning, Oct. 20th, an earthquake vibration was felt throughout Canada, and the northern part of the United States from Maine to Iowa. It seems to have been more severe in Canada and in New England. In many places the shock was sufficient to throw down chimnies, crack the walls of buildings, and do other damage. It was remarkably severe for the region of country visited.

At New Haven, as well as in many other places, there were two distinct series of vibrations. Prof. Twining has carefully collected information from several persons as to the time of the occurrence and duration of the vibrations. The beginning of the first shock was at $11^h\ 19^m\ 45^s$ A. M. New Haven mean time. It lasted ten seconds, and its individual vibrations were about two-thirds of a second in duration, or one and one-third second for a complete double vibration. After an interval of five seconds there was a second series like the first, lasting eleven seconds.

The motion was not a simple oscillation but there was a rocking motion, indicating a vertical component in the movement of the earth. The vibrations were not severe enough to arrest universal attention, though multitudes felt a peculiar sensation without recognizing the cause. The direction of the vibration was N.N.E. and S.S.W.

At Cambridge, Mass., according to Prof. Winlock, the direction was about 10° north of east, as determined by the appearance of the sides of a vessel containing milk.

Mr. Farmer, at Boston, gives $11^h\ 25^m\ 37^s$ for the time of the ending of the vibrations, Cambridge mean time. This would imply that the shock reached Boston a minute and three-fourths earlier than New Haven.

At Cleveland, Ohio, several clocks were stopped by the earthquake, each indicating very nearly $10^h\ 45^m$ A. M. This is approximately the instant at which the shock reached New Haven.

It is reported that the shock reached Quebec 30 seconds before it did Montreal. the telegraph operator of the former city being in the act of inquiring of the operator in the latter one respecting the earthquake when it arrived at Montreal. These data seem to show that the general progress of the wave was from North to South.

Slight vibrations were felt as far south as Richmond, Va., and as far west as Dubuque, Iowa. H. A. N.

3. *Kansas Natural History Society.*—(From the Secretary of the Society.)—The third annual meeting of this Society was held in Lawrence, Kansas, the first week in September, in the University Building. Gen. John Fraser, President of the State University, delivered a lecture, the first evening of the session, "On the Aims, Organization and Advantages of Scientific Associations," in which he recommended an enlargement of the scope of the Society to cover the whole field of scientific observation and investigation. On the following evening Rev. John H. Barrows, of Arvonia, delivered an address in the Presbyterian church on "Hugh Miller, or The Workingman's Education." At the close of the lecture President Fraser, who spent his youth in Cromarty, gave some personal recollections of the great geologist.

The following papers were read before the Society: "A Catalogue of all the Plants of Kansas, as far as observed," by Prof. J. H. Carruth, of Lawrence. The catalogue contains about 500 species—30 being added by members of the association. "On the Fishes of the Kansas River, as observed at Lawrence," by Prof.

F. H. Snow, of Lawrence. This paper describes 27 species of fish. "On the Internal Heat of the Earth," by Rev. John D. Parker, of Burlington. "On a Comparison of the Coals of Kansas with other Western Coals," by Prof. Wm. H. Saunders, M.D., of Lawrence. "On the Saurian Formation and Moss Agate of Kansas," by Prof. B. F. Mudge, of Manhattan. Prof. Mudge exhibited to the Society some fine specimens gathered during his recent visit to the Rocky Mountains, among which was a fine, well-preserved Saurian.

Officers of the Society for the current year: Gen. John Fraser, President; B. F. Mudge, Vice President; John D. Parker, Secretary and Librarian; Frank H. Snow, Treasurer; B. F. Mudge and F. H. Snow, Curators. Curators of Departments: B. F. Mudge, Geology; F. H. Snow, Entomology; Wm. H. Saunders, Chemistry; John D. Parker, Meterology; J. H. Carruth, Botany.

4. *Corrections of errata in the "Notes on the structure of the Crinoidea, &c.*"—In page 226, in the 20th line from the top, for "*Spœnonites*," read "*Sphœronites*;" page 228, in the 5th line, for "Miller," read Müller;" page 230, in the 17th line for "ovo-anal," read "oro-anal;" in page 232, for "Crinoidea," read "paleozoic Crinoidea;" and in page 233, 11th line, for "interradial," read "internal." I desire also to express my obligations to Prof. Alexander Winchell, State Geologist of Michigan, and to Mr. Charles Wachsmuth of Burlington, Iowa, for their kind assistance in lending me a large number of beautifully preserved specimens.

E. BILLINGS.

OBITUARY.

Capt. James Pedersen, whose name is associated with numerous new and rare species of corals, echinoderms, shells, etc., described in this Journal and elsewhere, as well as with the very extensive collections which he has contributed to the Museum of Yale College, and of which portions have been distributed to many other Museums, both in this country and in Europe, died in San Francisco, Aug. 19th. He was born at Christiania, Norway, February 24th, 1811. His father, Sars Pedersen, and his ancestors for several generations, were sea-faring men, and he seems to have early developed the same taste. In 1822, and for several years subsequently, he sailed in his father's vessels between Christiania and Copenhagen, and by his visits to the celebrated museums and galleries of the latter city, he acquired a taste for the rare and curious in nature and art, which in later years led him to become an enthusiastic and intelligent collector of objects of Natûral History. In 1828–9 he studied navigation and took his diploma, and afterward sailed to St. Petersburg. In 1830 he came to this country, and subsequently made New York his home. He sailed for 19 years between New York and Havana, New Orleans, Vera Cruz, Honduras, etc., but in 1849 he took his vessel to California, and since then has spent most of his time in various parts of the Pacific and Indian Oceans, being for some time on the South American coast, and some years in China and Japan. The last years of his life were spent at La Paz, Lower California, where he engaged to a considerable extent, in the pearl fishery, and from

whence he sent the extensive zoölogical collections which adorn the Museum of Yale College. These collections are no doubt the largest that have ever been made in the Gulf of California, and when fully described will add greatly to the knowledge of that rich fauna. He was obliged to leave La Paz last December, on account of failing health and the excessive heat, but after his arrival in San Francisco, was still mindful of the interests of science, and continued to send valuable collections as often as he was able. He was a thoroughly temperate, Christian man, who has done much in his humble way, to advance science. His collections will be an enduring monument to his memory.

Dr. W. A. MILLER, the author of "Miller's Elements of Chemistry," and of various memoirs on chemical subjects published in the Philosophical Transactions and elsewhere, died at Liverpool, of apoplexy, on the 30th of September. He was born at Ipswich in December, 1817. In 1861 he was elected Treasurer of the Royal Society. He occupied for some years the office of President of the Chemical Society, and was one of the assayers to the Royal Mint.

Dr. A. MATTHEISSEN, a chemist of high promise, and one of the examiners of the University of London, died recently, in his 39th year.

VI. MISCELLANEOUS BIBLIOGRAPHY.

1. *Transactions of the Connecticut Academy of Arts and Sciences*, Vol. II, Part I. New Haven, August, 1870. 210 pp. 8vo, 7 lithog. plates. Price $3.—This number includes: Notice of the Crustacea collected by Prof. C. F. Hartt on the coast of Brazil in 1867, by Sidney I. Smith; On the Geology of the New Haven Region, with special reference to the origin of some of its topographical features, by James D. Dana; Notes on American Crustacea, No. 1, Ocypodoidea, by S. I. Smith; On some alleged specimens of Indian Onomatopœia, by J. H. Trumbull; On the Molluscan fauna of the later Tertiary of Peru, by E. T. Nelson. Several of these papers have already been noticed when the author's copies have been distributed. The last, illustrated by two plates, is of special interest as throwing some light on the ancient marine fauna of the Pacific coast of Central America, and its relations to that of the existing faunæ of the same coast and of the Atlantic coast, thus aiding in solving the question of a connection, more or less ancient, between the two oceans across the Isthmus. In this paper numerous new species are described from a formation very rich in fossils near Zorritos, Peru. The formation is probably of Miocene or even earlier age, since only one or two species appear identical with living forms, but the genera are nearly all identical with the common genera now living on the same coast, and the species are often representative of or analogous to living forms of the same region, being apparently much more nearly related to the existing species of Panama, than to either the Miocene or living species of the West Indies. v.

2. *A Treatise on Elementary Geometry;* by Prof. WILLIAM CHAUVENET. Lippincott & Co., Philadelphia.—The reputation of Prof. Chauvenet in science, and the knowledge of his experience as a teacher, will draw peculiar attention to this work upon Elementary Geometry.

In the methods of demonstration Prof. Chauvenet resembles Legendre more than Euclid, though he differs widely from both. He uses the *reductio ad absurdum* rarely, and the various modes of superposition quite extensively. In his treatment of ratios he uses algebraic symbols. The difficulties of incommensurable ratios are met by the early introduction of the idea and theory of limits. He does not adopt the *direction* theory of parallels, believing that it does not meet all difficulties, especially in solid geometry. He introduces the idea of *loci* early in the first Book. To *symmetrical* figures he gives special place and importance. He adds two appendices, the first containing exercises upon the several books, and the second an introduction to modern geometry.

As a preparation for the study of the theory of transversals and other branches of modern Geometry, the introduction of algebraic symbols in elementary demonstrations is of great value. The modern Geometry has greater power than that of Euclid for two reasons: first, it uses the *plus* and *minus* of algebra; and second, it implicitly uses the roots of quadratic equations, real and imaginary.

On the other hand, the use of algebraic symbols in elementary geometry, has, we believe, a disadvantage. The statement of the steps of the syllogism is not so lucid.

The introduction of the idea of limits early in geometry is a good feature of the work. Students have in decimal fractions and their applications been accustomed to incommensurable ratios, and we believe that such early use is not premature.

For the sake of discipline in clear and distinct statement and close consecution of thought, we admit a special liking to the first six books of Euclid. But Prof. Chauvenet's work more nearly satisfies our idea of a good geometry than any other modern text-book. H. A. N.

3. *Grundzüge einer allgemeinen Theorie der Oberflächen in synthetischer Behandlung;* von LUDWIG CREMONA. Translated into German by M. Curtze. Berlin, 1870. (S. Calvary & Co.)—This volume is ostensibly a translation of the memoirs of the distinguished Italian Geometer, but it is in fact much more. Rather more than half of the volume is new matter, added by Prof. Cremona, or under his supervision.

The work is an extension to surfaces of Prof. Cremona's method of dealing with plane curves, as shown in his memoir published by the Bologna Academy, *Introduzione ad una teoria geometrica delle curve piane.* This important work had previously been translated by M. Curtze into German.

These German translations render Cremona's important contributions to pure Geometry much more accessible than they are in the original Italian. H. A. N.

4. *A historical and descriptive narrative of the Mammoth Cave of Kentucky*, including explanations of the causes concerned in its formation, its Atmospheric conditions, its Chemistry, Geology, Zoology, etc.; with full scientific details of the eyeless Fishes; by W. Stump Norwood, M.D. 225 pp. 12mo, with illustrations. Philadelphia, 1870. (J. B. Lippincott & Co.)—All future visitors to the Mammoth Cave will have occasion to thank Dr. Norwood for the service he has rendered the tourist. The book contains several interior views of the cave, which give some idea of the wonders within, although a very inadequate one. They should be supplemented by a good map of the extensive windings of the cavern. The author estimates the extent of the various avenues and mazes of the Mammoth Cave at over one hundred and fifty miles. An instrumental survey would probably shorten this assumed distance, but we know from our own wanderings in it that its extent is immense. The chapter on the eyeless fishes is made up mostly from the writings of Agassiz and Wyman, and presents the facts on the subject in an attractive form, well adapted to instruct and interest the general reader.

5. *Tent life in Siberia, and Adventures among the Koraks and other Tribes in Northern Kamtchatka and Northern Asia;* by George Kinnan. 425 pp. 12mo, with a map. New York, 1870. (G. P. Putnam & Sons, London; S. Low, Son and Marston). —A very readable and instructive narrative of travel in a little known region of the globe, is this neat volume of Mr. Kinnan's. The costly and disastrous enterprise of the Western Union Telegraph Company, designed to establish electrical communication between America and Asia through Kamtchatka and Northern Asia, has been by no means fruitless in scientific interest. Already Messrs. Whymple and Dall have given us instructive and valuable contributions upon their wanderings in British Columbia and Alaska. Mr. Kinnan makes no claim to any special devotion to scientific investigation, but his narrative gives a clear and lively account of the physical and social conditions of a country rarely visited, and full of curious interest. His volume well repays perusal, and is obviously the work of a good observer and careful historian, who is never tedious, and has the agreeable art of carrying his reader along with him.

6. *Report of the Superintendent of the U. S. Coast Survey, showing the progress of the Survey during the year* 1867. 344 pp. 4to, with 28 maps.—This volume, besides the Report of the Superintendent, Prof. Peirce, contains, among the articles in its Appendix, a Report on Transatlantic determination of Longitude, by Dr. B. A. Gould; on comparison of Meters, by Dr. F. A. P. Barnard and M. Tresca; on a new form of Reflector for geodetic signals, by J. E. Hilgard; on the Tides and Currents of Hell Gate, by Henry Mitchell; on Soundings in the Gulf Stream, by H. Mitchell; on the Fauna of the Gulf Stream, by L. F. Pourtales; on Alaska Territory, by G. Davidson; obituary of Alexander Bache.

7. *Treatise on the power of Water as applied to drive Flour Mills and to give motion to Turbines and other Hydrostatic*

Engines; by JOSEPH GLYNN, F.R.S., member of the Institute of Civil Engineers of London, etc. 3d ed., revised and enlarged. 162 pp. 12mo, with numerous illustrations. New York, 1869. (D. Van Nostrand).—This is a popular and practical treatise on water as a motive power. It is simple in its explanations and mathematics, and is well illustrated.

8. *Archives of Science and Transactions of the Orleans Co. Society of Natural Sciences.*—This first number of the Archives of a new Natural History Society, at Newport, Vermont, contains the following papers: On the characters and customs of the Pawnees, by Rev. T. E. Ranney; Qualitative Analysis of the Mineral Springs of Essex Co., Vt., by H. A. Cutting; on the Indian History of Northern Vermont, by Wm. W. Grout, Esq.; Meteorological register, by J. M. Currier; on a new mounting for microscopic objects, H. A. Cutting.

9. *The American Entomologist and Botanist.*—This St. Louis Journal will be suspended during the year 1871, to be resumed with the commencement of the following year.

10. *Geology and Revelation, or the Ancient History of the Earth, considered in the light of Geological facts and Revealed religion,* with illustrations; by the Rev. GERALD MOLLOY, D.D., Prof. Theol. Roy. Coll. St. Patrick, Maynooth. 380 pp. 12mo. New York, 1870. (G. P. Putnam & Sons.)—A handsome American edition of the excellent work noticed at page 151 of this volume.

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Results of Astronomical and Meteorological Observations made at the Radcliffe Observatory, Oxford, in the year 1867, under the superintendence of Rev. Robert Main, M.A., Radcliffe observer. Vol. xxvii.

The Portable Transit Instrument in the Vertical of the Pole Star; translated from the original memoir of Wm. Döllen, by Cleveland Abbe, Director of the Cincinnati Observatory. 48 pp. 8vo. Washington, 1870.

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GENERAL INDEX

OF

VOLS. XLI—L, SECOND SERIES.

A

Abbe, C., connection between terrestrial temperature and solar spots, l, 345.
Repsold Circle, iii, 207, 309.
Abel, F. A., gun-cotton, iv, 288.
Academy, American, Proceedings, iii, 140; iv, 296; v, 428.
memoirs, v, 428.
medal, ii, 136.
notice of, Bentham, iv, 302.
California, Proceedings, iii, 424; v, 144; l, 296.
memoirs, v, 267.
Chicago, museum burned, ii, 135.
notice of, i, 281.
proceedings, ii, 140.
transactions, v. 418, 427; ix, 285.
National, i, 141, 430;
annual of, iv, 295.
meetings, ii, 431; iii, 419; iv, 435; v, 280; vi, 428; ix, 439.
memoirs, ii, 287; iii, 140.
Philadelphia, proceedings, i. 144, 431; ii, 140, 292; iii, 140, 423; iv, 296; v, 144, 428; vi, 152; vii, 152; viii, 298, 451; ix. 444; l, 296, 440.
Paris, new members, ix, 284.
St. Louis, Transactions, ii, 140; vi, 288.
Academy, a new monthly, viii, 452.
Acclimatization of ostrich, i, 109; v. 72.
of the salmon, i, 109.
in Australia, iii, 97.
Acetyl compounds of carbohydrates, *Schützenberger* and *Naudin*, viii, 414.
Acetylene, polymers of, *Berthelot*, iii, 389.
Acids, aromatic, synthesis of, *Wurtz*, l, 115.
new mixed, *Gal*, iv, 104.
normal and derived. *Barker*, iv. 384.
of the lactic series, *Frankland* and *Duppa*, iv, 268.
Acids, organic, synthesis of, *Carius*, iv, 265.
Adam, Tableau mineralogique, ix, 119.
Adams, J. H., minerals at Pelham, ix, 271.
Adhesion of vapor, *Gibbs*, iv, 101.
Æpyornis of Madagascar, ix, 275.
Aerolites, see *Meteorites*.
Africa, journey into, *Du Chaillu*, i, 281.
telegraph by sound, i, 140.
African explorations, Livingston's, ix, 14.
Agassiz, A., on Acalephæ, noticed, i, 431; ii, 132.
notice of Edwards's Oiseaux fossiles de la France, vii, 276.
Embryology of Starfishes, ii, 134.
young stages of Annelids, ii, 427.
Agassiz's Geological Sketches, noticed, i, 407.
Journey in Brazil, noticed, v, 288.
Agricultural chart by Delesse, i, 429.
Agriculture, Report of Commissioner of, 1864, i, 430.
Ainos of Yesso, *Bickmore*, v, 353.
of Saghalien and Kurile Islands, v, 361.
Air-rings, visible, *Ball*, vi, 260.
Albertite, origin of, *Peckham*, viii, 362.
Alcohol from castor oil, vii, 426.
normal amylic, l, 416.
iso-butyl, conversion of, into tertiary pseudo-butyl alcohol, l, 114.
octylic, new source of, vii, 425.
tritylic, by fermentation, vii, 426.
Alcohols, and chlorinated ethers, iii, 249.
action of on terchlorid of phosphorus, iii. 389.
Aldehyd. methylic, *Hoffman*, v, 249.
Alexander, W. D., crater of Haleakala, ix, 43.
Algæ in hot springs, vi, 131.
in N. Atlantic ocean, *Dickie*, l, 428.

Alkalies, test for, v, 255.
Alkaloid, on a new, *Rosenstiehl*, viii, 123.
Allen, H., Comparative Anatomy and Medical Zoology by, noticed, vii, 149.
Allen, J. A., Catalogue of Massachusetts Reptiles and Batrachians by, noticed, vii, 285.
Eared Seals, noticed, l, 431.
Mammals of Massachusetts, ix, 134.
Alloys, Japanese, *Pumpelly*, ii, 43.
Almanac, Amer. Nautical for 1869, v, 284.
Altitudes in British America, iv, 115.
of Mount Hood, iv, 429.
of Mount Washington, iv, 377.
Amalgams, *Nicklès*, iii, 96.
America, Rocks of, *Catlin*, l, 135.
American Association Adv. Sci., meeting of, ii, 287; iv, 137, 431; viii, 146, 289; l, 150, 286.
Proceedings, 1866, v, 144; 1866, vi, 151; 1867, vi, 287; 1868, vi, 148, 275.
Chemist, noticed, l, 295.
Entomologist, noticed, l, 282.
Naturalist, iii, 136, 284; iv, 139; vi, 286.
Philosophical Soc., Philad., Proceedings, iii, 140, 424; vii, 296; ix, 288.
Transactions, i, 144.
Ammonia, nitrite of, *Loew*, vi, 29.
Ammonia-chromium bases, *Cleve*, ix, 251
Ammonium-amalgam, *Pfeil* and *Leffman*, ii, 72.
composition of, vii, 420.
Amyl, arsenate of, l, 14.
arsenite of, l, 20.
Amylene, relation of oil of turpentine to, *Bauer* and *Verson*, viii, 416.
Analyses of shells, *How*, i, 379.
Analysis for chlorine, *Warren*, ii, 156.
new method of volumetric, *Gibbs*, iv, 207.
of petroleum, apparatus for, *Peckham*, iv, 230.
organic, determination of oxygen in, v, 255.
chemical, repetition in, *Godwin*, l, 249.
spectroscopic, see *Spectroscope*.
use of sand and glass filters in quantitative, *Gibbs*, iv, 215.
Will's tables for Quantitative, noticed, iv, 140.
Analytical processes, new, *Talbott*, l, 244.
Andes and the Amazon, by Orton, noticed, l, 294.
of Quito, phys. geog. of, v, 99.
of Equador, geology of, *Orton*, vii, 242.
observations on, *Orton*, vi, 203.
Andrews, E., glacial drift of Illinois, iii, 75.
note on, *Hilgard*, iii, 241.
Andrews, E., Human Antiquities at Abbeville, etc., v, 180.
Lakes of N. America as chronometers, l, 264, 424.
on western Boulder drift, viii, 172.
Andrews, E. B., Geological relations of petroleum, ii, 33.
Angström. on violet spectrum, v, 250.
Aniline, *Reimann*, ix, 142.
Animals, commensalism among, l, 285.
Annual of Scientific Discovery, vii, 434.
Annulata, on recent works on, viii, 199.
Ansted's Physical Geography, v, 143.
Anthony, W. A., bones of rattlesnakes, ii, 285.
Antimonous sulphid, preparation of, *Sharples*, l, 248.
Antimony, detonating, i, 107.
sulphates of oxyd of, *Dexter*, vi, 78.
Antozone, Meissner on, l, 213.
Apparatus for studying absorption, l, 52.
Appleton's Amer. Cyclopedia, ii, 138; iv, 140; vi, 432.
Aquitanian remains, *Lartet*, noticed, ii, 291.
Aral Sea, changes in, *Rawlinson*, iv, 133.
Archæology of Southern France, Lartet and Christy on, noticed, iv, 119.
prehistoric in Greece, *Finlay*, l, 251.
Arctic researches, recent, *Gilman*, vii, 103, 377.
Arents, A., partzite, iii, 362.
Arithmetic, Eaton's, noticed, iv, 140.
Arizona, *B. Silliman*, i, 289.
Arnott, G. A. W., obituary, vi, 273.
Gray, vii, 240.
Arsenic, allotropic, v, 254.
and arsenous acid ethers, *Crafts*, l, 10.
nitrate of silver test for, vii, 255.
Artemia, new localities of, *Verrill*, viii, 430.
Artesian water, Charleston, S. C., vii, 357.
well of Terre Haute, Ind., viii, 270.
Assmuss on products of distillation, *Ordway*, v, 274.
Association, see *American* and *British*.
Asphalt, vein in Virginia, i, 120.
Aspirator, form of, *Leeds*, v, 423.
Asteroid (85), elements of, *Peters*, i, 277.
(86), Semele, elements, ii, 134.
(87), ii, 135.
(90), Antiope, iii, 279.
(91), iii, 279.
(91), Ægina, vi, 147.
(93), Minerva, vi, 147.
(94), Aurora, vi, 147.
(95), Arethusa, vi, 147.
(96), vi, 147.
(97), Clotho, vi, 147.
(98), vi, 274.
(99), vi, 274.
(100), Hecate, vi, 393.
(101), Helena, vi, 274, 393.

Asteroid (102), Miriam, vi, 275, 392, 393.
(103), vi, 392, 393.
(104), vi, 392, 393.
(105), vi, 392, 393.
(106), vi, 393.
(109), elements of, *Rogers*, ix, 141, 428; *Peters*, ix, 277.
(111), l, 285, 432.
(112), Iphigenia, l, 409.
Astro-meteorology, recent contributions to, *Newton*, iii, 285.
Astronomical observations, *Gilliss*, 1864, iii, 413.
Observatory, Russian, Director of, ii, 286.
of Harvard College, Annals of, noticed, iv, 293.
Society, abstracts of Reports of, ix, 431.
Astronomy, Chambers's, noticed, iii, 422.
Kirkwood's Meteoric, noticed, iv, 428.
Lockyer's, noticed, vi, 275.
Norton's, noticed, iv, 295.
Snell's Olmsted's, noticed, ii, 139.
Watson's, noticed, *Newton*, vi, 145.
see further, *Star*.
Astro-photometer, *Zollner* on, ii, 418.
Athyris, subdivisions of, *Billings*, iv, 48.
Atlantic Telegraph. Fields's history of, ii, 437.
Atmospheric system as God made it, by Butler, noticed. l, 150.
Atomic ratio, *Cooke*, vii, 386.
weights, determination of by optical means, *Schrauf*, iv, 113.
volumes of elements, *Clarke*, vii, 180, 308.
solid compounds. *Clarke*, l, 174.
Atomicities, theory of, *Gibbs*, iv. 409.
Atoms, chemical, existence of, *Kekulé*, iv, 270.
size of, *Thomson*, l, 38.
Atwater, W. O., on American maize, viii, 352.
Audubon, life of, noticed, ix, 443.
Aureole observed in solar eclipse, viii, 434.
Auroral appearances connected with phenomena of terrestrial magnetism, *Stewart*, ix, 281.
displays, recent, *Newton*, l, 146.
Aurora at Toronto, *Kingston*, viii, 65.
Bandelier, iii, 279.
magnetic effects of, *Farmer*, i, 118.
of April, 1869, *Gilman*, viii, 114.
of April 15, 1869, viii, 146.
spectrum of, viii, 404.
Auroras and Sun-spots compared with magnetic declination, *Loomis*, l, 153.
Sept., 1868, in Maine, *Gilman*, vi, 390.
Australia, diamonds in, ix, 275.
Dinornis in, ix, 273.
Australia, gigantic marsupials of, i, 258.
Lias and oolite of, *Moore*, l, 269.
Smyth's gold-fields of Victoria, ix, 263.
tall trees in, iv, 422.
Victoria, mineralogy of, *Ulrich*, noticed, l, 271.
works on Botany of, noticed, i, 415.
Avery, C. E., test for arsenic, vii, 255.

B

Babinet, undulatory theory of heat, iv, 111.
Babington, revision of flora of Iceland, l, 277.
Bache, A. D., magnetic observations in Maine, ii, 141.
obituary of, iii, 282.
Bahr, on yttria and erbia, i, 399.
Baillon's Histoire des Plantes, noticed, l, 276.
Baird, S. F., distribution and migration of North American birds, i, 78, 184, 337.
Baird's American Birds, noticed, ii, 134.
Baker, J. G., Flora Brasiliensis, Martius, noticed, l, 425.
genera, etc., of herbaceous liliaceæ, l, 427.
Ball, R., air-rings, vi, 260.
Bandelier, A. F., aurora, iii, 279.
Barker, G. F., chemical abstracts, v, 251; vi, 129; vii, 420; viii, 123, 408; l, 111, 255.
formic vs. carbonous acid, iv, 263.
normal and derived acids, iv, 384.
notices in physiological chemistry, vi, 233, 379; vii, 20, 258, 393; viii, 49.
Roscoe's Chemistry, iii, 137.
Frankland's Chemistry, noticed by, iii, 137.
silvering on glass, iii, 252.
abstract of Meissner's research on electrized oxygen, l, 213.
Barium chlorate, *Brandau*, l. 111.
Barometer, automatic, *Hough*, i, 43.
Barometric pressure, distribution of, ix, 440.
Barnard, F. A. P., Industrial Machinery and Scientific Apparatus by, noticed, viii, 297.
Machinery of Industrial Arts, etc., ix, 175.
Barrande, J., Arethusina, Faune Silurienne, noticed, viii, 138.
Ptéropodes Siluriens de la Bohême, v, 120.
Batrachia and Reptilia, Synopsis of, *Cope*, noticed, l, 150.
Bayma's Mechanics, *Norton*, vi, 167.
Beaver, remains of in New Jersey, *Weld*, viii, 433.

Becquerel, Elémens d'électro-chimie, v, 75.
Belgian cave-remains, *Dupont*, iii, 121, 260.
Bennett, the Genus Hydrolea, l, 277.
Bentham's addresses before the Linnean Society, i, 410; iv, 297; l, 279, 325.
Bentham's Flora Australis, iii, 410.
Bentham and Hooker, Genera Plantarum, vol. I, noticed, *Gray*, v, 271, 404.
Benzole, etc., action of heat on, *Berthelot*, iv, 266.
Berg, O., obituary, v, 124.
Bergé, Le Chimiste, noticed, iii, 99.
Berthelot, action of heat on benzole and analogous hydrocarbons, iv, 266.
hydrocarbons in coal-tar, iv, 418.
on isomerism, ii, 257.
metals in organic radicals, ii, 256.
on carbonylic sulphid, vi, 129.
on cyanhydric acid, vii, 420.
on saturating organic bodies with hydrogen, vi, 125, 395.
on varieties of carbon, vii, 418.
on synthesis of organic acids, ix, 388.
synthesis of oxalic acid, v, 249.
hydrocarbons, iii, 96, 251, 389.
reductions in chemistry, iii. 386.
Bessemer, manufacture of cast-steel, i, 278.
flame, spectroscopic examination of, *J. M. Silliman*, l, 297.
Bickmore, A. S., Ainos of Yesso, v, 353, Saghalien. etc., v, 361.
recent geological changes in China and Japan, v, 209.
journey through China, vi, 1.
Bigsby's Thesaurus siluricus, vi. 435.
Bill, J. H., test for bromids, v, 224.
Billings, E., work on fossils of Canada, i, 124.
Fossils of Anticosti, noticed, iii, 137, 259.
on Crinoidea, Cystidea, and Blastoidea. viii, 69; ix, 51; l, 225, 435.
subdivisions of Athyris, iv, 48.
structure of Blastoidea, vii, 353.
Worthen's Illinois survey, iii, 395.
Binney's Gould's Invertebrata of Mass., noticed, ix, 423.
Binocular vision, *Le Conte*, vii, 68, 153.
Birds, carbolizing, *Parker*, l, 283.
distribution and migration of, *Baird*, i, 78, 184, 337.
of North America, *Verrill*, i, 249.
Baird's N. American, ii, 134, 291.
Birt, W. B., obscuration of "Linné," iii, 411.
Bissell, S. R., St. Thomas earthquake, v, 133.
Blake, E. W., on figures produced by electric spark, ix, 289.
Blake, J. M., on measuring angles of crystals, i, 308.
Gay-Lussite from Nevada, ii, 221.
natural terpin, iii, 202.
on kaolinite and pholerite, iii, 351, 405.
on hortonolite, viii, 17.
Blake, T. A., Northwest coast of America, v, 242.
Blake, W. P., crystallized gold in California, i, 120.
fossils in Sierra Nevada, i, 406; in California, iii, 270.
fossil tapir in California, v, 381.
tooth from Table Mt., l, 262.
glaciers of Alaska, iv, 96.
gold-bearing rocks of California, v, 264.
Minerals of California, noticed, ii, 114, 125.
of Paris exposition, v, 194.
mineralogical notices, iii, 124.
on decrease in production of gold, vii. 432.
Report on the precious metals by, noticed, viii, 126.
Bliss, J. S., Wisconsin drift, i, 255.
Blomstrand, conjugate bodies in inorganic chemistry, ix, 110.
Blunt, E., obituary, ii, 433.
Bogota, S. A., earthquake at, *Hurlbut*, l, 408.
Boissier's Icones Euphorbiarum, ii, 427.
Bolton, H. C., on action of light on uranium, viii, 206.
Bone-caves of Brazil, *Reinhardt*, vi, 264.
Gibraltar, Report on, vii, 277.
see MAN.
Boott's Illustrations of Carex, v, 271, 409.
Borax in California, *Whitney*, i, 255.
Boron, combinations with halogens, i, 108.
graphitoidal, *Deville*, iii, 250.
Boston Society Natural History, Proceedings, i, 144, 431; ii, 140, 292; iii, 140, 423; iv, 144, 296; v, 144. 428; vi, 152. 288; vii, 152, 296, 436; viii, 452; ix, 288, 444.
condition and doings of, iv, 437.
memoirs, iv. 205; vi, 152.
Botanical Congress, London, i, 140; ii, 129.
Address to, *De Candolle*, ii, 230.
necrology, 1865, i. 263; 1868, *Gray*, vii, 140; 1869, ix, 129.
Club, bulletin of the Torrey, ix, 404.
nomenclature, *De Candolle*, l, 274.
Notabilia, *Gray*, ix, 120.
BOTANICAL NOTICES:—
Anderson, Monographia Salicum, iv, 420.
Annales Musei Botanici Lugduni-Batavi, iv, 424.

BOTANICAL NOTICES:—
Bentham, Flora Austral., iii. 410.
Bentham and *Hooker's* Gen. Plantarum, i, 132.
I, *Gray*, v, 271, 404.
Boissier, Flora Orientalis, iv, 122.
Icones Euphorbiarum, ii, 427.
Boott, Illustrations of carex, v, 271, 409.
Boussingault on action of foliage, ii, 126.
Brown R., works, iii, 125.
vol. II, vi, 271.
Brunet, Eléments de Botanique, noticed, l, 278.
Végétaux ligneux du Canada, iv, 122.
Bunge, on genus Cousinia, i, 416.
Generis Astragali, etc., l, 279.
Curtis, esculent Fungi of United States, ii, 129.
Botany of North Carolina Survey, v. 271.
Darwin, on climbing plants, i, 125.
Daubeny's Trees and Shrubs of the ancients, i, 268.
De Candolle, A., Lois de la Nomenclature, iv, 421.
Prodromus, ii, 427; vi, 408.
De Candolle, C., Piperaceæ, iii, 128.
Théorie de la Feuille, vi, 272.
Eichler, on morphology of the Androecium in Fumariaceæ, i, 412.
Englemann, Juncus, species of, in N. America, ii, 128.
Flora Brasiliensis, i, 412; ix, 404.
Caucasi, Rupprecht, l. 279.
of British Is., *Hooker*, noticed, l, 281.
of Iceland, revision of, l, 277.
Fournier on Cruciferæ, ii, 227.
Gray, A., Field, Forest, and Garden Botany, vii, 280.
Hand-book of British Algæ, ii 281.
Manual, iv, 284.
5th ed., 2d issue, v. 409.
Griseback, Catal. Plant. Cubens., iii, 409.
Henfrey, Elementary Botany, noticed, l, 429.
Hooker, Botanical works of, i, 2.
and *Baker*, Synopsis Filicum, vii, 143.
Hoopes, Book of Evergreens, vi, 270.
Krok, on Valerianeæ, i, 416.
Le Maout and *Decaisne*, Traité de Botan., v, 409.
Lesquereux, Catalogue of Mosses, v, 273.
Linnean Soc. Journal, vi, 271.
Lyell, Hand-book of Ferns, ix, 404.
BOTANICAL NOTICES:—
Martius, v., Flora Brasiliensis, iv, 421; vi, 408; ix, 404; l, 425.
Herbarium of, l, 279.
Miquel, affinités de la flora du Japan avec celles de l'Asie et de l'America du Nord, iv, 421.
Prolusio Floræ Japonicæ, etc., v, 403.
Moigno, growth of Lycoperdon giganteum, iv, 123.
Müller, Botany of Australia, i, 415.
vegetation of the Chatham Islands, i, 416.
Musci Boreali Americana, i, 417.
Paine's Catalogue, i, 130.
Parlatore, Botanical method, iii, 273.
Coniferæ, iii, 272.
species of Cotton, iii, 272.
Report of International Horticultural Exhibition and Botanical Congress, iv, 123.
Salisbury, Genera of plants, ii, 280.
Schmidt, Reisen in Amur-lande, vi, 408.
Seemann's Flora Vitiensis, i, 414.
Vilmorin-Andrieux & Cie, Fleurs de pleine terre, v, 269.
Watson, Index to names of Indian plants, vii. 143.
Wimmer, Salices Europeæ, iii, 272.
BOTANY:—
Algæ from hot springs, *Wood*, vi, 31.
American heather, *Gray*, iii, 128.
Anthers, *Müller*, iii 126.
Autumnal foliage, *Wharton*, vii, 251.
Branches, changes of direction of, ii, 130.
Caricography, *C. Dewey*, i, 226, 326; ii, 243; index to, ii, 325.
Cinchona, species of, ii, 131.
Corydalis cava, fertilization of, ii, 131.
Cyperaceæ, *Boeckeler*, l, 279.
Development of flower of Pinguicola vulgaris, ix, 404.
Dimerous flower of Cypripedium candidum, ii. 195.
Fertilization of flowers by insects, *Ogle*, l, 278.
Flora of Eastern Kansas, *Hall*, l, 29.
Hydrolea, the genus, *Bennett*, l, 277.
Ipomea simulans, *Hanbury*, l, 277.
Jalap, Tampico, l, 277.
Lauraceæ, *Clarke* on, ii. 131.
Lessingia Germanorum, ii, 128.
Life in the hot and saline waters in California. *Brewer*, i, 391; ii. 429.
Mountain plants, migration of, ii, 132.
Nomenclature, laws of, at Botanical Congress, vi, 63.
remarks on, *Gray*, vi, 74.

BOTANY:—
Notes on vegetable physiology, including effects of light, modes of fertilization. etc, ix, 405.
Oaks, Michaux Grove, l, 279.
Orotava dragon-tree, v, 270.
Palm culture, *Wendland*, ii, 132.
Plantæ Wrightianæ Cubenses, vii, 144.
Scolopendrium officinarum, *Paine*, i, 417; ii, 281.
Sequoia of California measured, ii, 129.
Sisymbrium, *Gray*, ii, 277.
Shortia and Schizocodon identical, v, 402.
Stamens, morphology of. iii, 273.
System in, *Kock*, ii, 132.
Tenacity of life of seeds and spores, i, 393.
Tree-labels, Creighton's, iii, 273.
Union of two trees, *Lyman*, iii, 275.
Boussingault, on action of foliage, ii, 126.
Brachiopoda, a division of annelida, *Morse*, l. 100.
Bradley, F. H., fish-remains in western New York, ii, 70.
Indiana geol. survey, l, 135.
Braithwaite's Retrospect, noticed. vi, 286.
Brandau, preparation of barium chlorate, l, 111.
Brande, W. T., obituary of, i, 428.
Braun, tests for glucose, iii, 250.
Brazil, reptilian remains. *Marsh*, vii, 390.
Bresse's hydraulic motors noticed, ix, 144.
Brewer, W. H., gold-rocks of Pacific coast, ii, 114.
gold-bearing rocks of California, v, 397.
Human skull found in California, ii, 424.
Mount Hood, ii, 422.
notice of Darwin's Variation under Domestication, vi, 140.
obituary of Martius, vii, 288.
on life in hot and saline waters in California, i, 391.
on tenacity of life in seeds and spores, i, 393.
review of Whitney's Report on California, i, 231, 351.
Brewster, D., obituary of, v, 284.
Brewster's neutral point, *Chase*, ii, 111, 112; iv, 70.
Brigham on Hawaiian volcanoes, noticed, v, 427.
Brines, chemistry of, *Goessmann*, iv, 77.
Brinton, D. G., on Central American manuscripts, vii, 222.
British Association, Address of T. H. Huxley, President, l, 383.
meetings of, ii, 433; iv, 436; vi, 148, 430; viii, 438; l, 150.
Proceedings of, i, 141.
British Islands, Student's flora of, noticed, l, 281.
Brodie, B. C., the new chemical calculus, iv, 270.
Bromhydric acid, prep. of, l, 113.
Bromids, test for, *Bill*, v, 224.
Brome, on Gibraltar caves, vii, 277.
Brown, R., Botanical works, noticed, vi, 271; iii, 125.
supposed absence of drift from Pacific slope of Rocky Mts., l, 318.
Browne, J. R., on mineral resources of the U. S., noticed, v, 439.
Browning, J., Spectra of meteors, v, 279.
Brunet, Eléments de Botanique, l, 278.
Brush, G. J., on Cookeite and Jefferisite, i, 246.
new mineral localities, ii, 268.
native hydrates of iron, iv, 219.
Kustel on ores, v, 287, 432.
on Sussexite, vi, 140, 240.
on Hortonolite, viii, 17.
on Durangite, viii, 179.
meteoric stone of Alabama, viii, 240.
on the magnetite in the Pennsville mica, viii, 360.
Bryant, C., habits of northern fur seal, noticed, l. 431.
Buchan, distribution of barometric pressure, etc., ix, 440.
Buchan's Meteorology, noticed, vii, 434.
Buckley's Texas Geol. Survey, ii, 437.
Buckton, double chlorids, etc., ix, 254.
Bunsen flame, theory of, *Knapp*, l, 255.
Bunsen, R., flame-reactions, iii, 110.
washing of precipitates, vii, 321.
Buoys, lighting power for, ix. 284.
Burkhardt, J., obituary, iii, 283.
Burton, B. S., enargite jamesonite, and tetrahedrite, v. 34.
Butler, T. B., Atmospheric system, noticed, l, 150.
Butterflies of North America, by Edwards, noticed, viii, 144, 433.

C

Cæsium, new salt of, *Sharples*, vii, 178.
Cahours, derivatives of tryethyl phosphines, l, 415.
Calculus, new chemical, *Brodie*, iv, 270.
Caldwell's Agricultural analysis, ix, 143.
California, *Blake's* list of minerals in, ii, 114, 125.
borax in, i, 255.
earthquake, 1868, vi, 428.
geological survey, i, 428; ix, 400.
gold-fields, geology of, vii, 134.
gold, crystallized mass of, i, 120.
human remains in, *Winslow*, i, 407.
skull discovered in, *Brewer*, ii, 424; iii, 265.

California, life in hot and saline waters of, i, 391; ii, 429.
new facts in geology of, *Whitney*, i, 252.
oils, distillation of, *Peckham*, vii, 9.
petroleum, see *Oil* and *Petroleum*.
publications of Geological survey of, viii, 133, 148, 151.
subdivisions of the Cretaceous of, *Gabb*, iv, 226; *Conrad*, iv, 376.
wealth of, Cronise on, noticed, vii, 150.
Whitney's geology of, reviewed, with notices of gold-rocks, i, 124, 231, 351.
Calomel, vapor-density of, vi, 398.
Cambridge course of Elem. Physics, v, 142; vi, 285.
Cambridge, Mass. Museum of Comparative Zoölogy, Report, noticed, vii, 286.
Campbell on the Northwest boundary, noticed, vii, 110.
Canals, between the Atlantic and Pacific, iv, 381.
Candles and soap, manufacture of, noticed, iv, 141.
Cannon, new kind of, *Treadwell*, i, 97.
strength of, *Treadwell*, v, 135.
Capitol at Washington, movement of dome of, by the winds, ix, 384.
Carbohydrates, acetyl compounds of, *Schützenberger* and *Naudin*, viii, 414.
action of water on, iii, 371.
Carbon, adamantine anthracitic, iv. 108.
bisulphid of, in sunlight, vi, 363.
in iron, how determined, vii, 374.
new sulphid of, i, 251.
replaced by silicon, ii. 255.
varieties, *Berthelot* on, vii, 418.
Carbonate, sodic, Kessler's patent, v, 254.
Carbonic acid, estimation of, *Johnson*, viii, 111.
Carbonous acid *versus* formic, *Barker*, iv, 263.
Carbonylic chlorid, *Schützenberger* on, vii, 423.
sulphid, *Than*, vii, 422.
Caricography, *Dewey*, i, 226, 326; ii, 243; index to, ii, 325.
Carius, synthesis of organic acids, iv, 265.
Carpenter, W. B., Eozoön Canadense, i, 406; vi, 245.
deep sea dredging, ix, 410.
on organic nature of Eozoön, iv, 367.
Carruthers on Equisetaceæ, vii, 279.
Carstanjen, E., thallic acid, iv, 269.
Caspary, on change of position in branches, ii, 130.
Cassin, J., obituary of, *Brewer*, vii, 291.
ornithological collection of, vii, 435.
Catlin, G., American Rocks, noticed, l, 135.
Caton, J. D., American Cervus by, noticed, viii, 144.
Caucasus, notes on, *von Koschkull*, vi, 214, 335.
highest peaks of, vii, 382.
Cave mammals, *Cope*, ix, 273.
Cavern, Malta, i, 140.
Central American MSS., *Brinton*, vii, 222.
Central Park of New York, report of commissioners of, noticed, vii, 445.
Cephalaspis, *Lankester*, i, 261.
Cephalization, No. IV, *Dana*, i, 163.
in mollusca, *Morse*, ii, 19.
Cerium, equivalent of, *Wolf*, vi, 53.
group, double sulphates of, ix, 356.
metallic, *Wöhler*, v, 254.
Chalk in Colorado, i, 401.
Chambers's Astronomy, iii, 422.
Encyclopedia, i, 288; ii, 139, 440; v, 143; vi, 433.
Champion, bromhydric acid, l, 113.
Chancourtois, origin of diamonds, ii, 271.
Chapman, E. J., on native lead northwest of Lake Superior, i, 254.
Chase, E. O., skylight polarization, iv, 265; v, 96.
Chase P. E., comparison of mechanical equivalents, l, 261.
connotations of magnetism, vi, 398.
laws of distribution of heat over the earth, iv, 68.
magnetism of iron, v, 247.
mechanical polarity, i, 90.
method of meteorological comparison, i, 158.
rain fall affected by the moon, vi, 281.
skylight polarization, ii, 111.
visibility of neutral points, ii, 112.
Chatard, T. M., gelatinous precipitates, l, 267.
Chatham Is., peat, etc., of, i, 123.
vegetation of, i, 416.
Chauvenet's Elementary Geometry, noticed. l, 438.
Chemical analysis; separation of lead and bismuth by bromo-thallates, i, 107.
on a process of elementary, *C. G. Wheeler*, i, 33.
repetition in, l, 249.
Sprengel pump in, ix, 378.
calculus, new, *Brodie*, iv, 240.
constitution and crystalline form, relation between, *Dana*, iv, 89, 252.
formulas of the silicates, iv, 252, 398.
History of six days of Creation, *Phin*, l, 294.
News, Am. ed., noticed, iv, 294; v, 144.

Chemical nomenclature, *Tillman's*, iii, 140.
philosophy, *Cooke*, noticed, vii, 435; l, 150.
synthesis, *Berthelot*, iii, 96, 251, 386. 389.
tables, *Sharples*, noticed, iii, 139.
CHEMISTRY:—
Acids, aromatic, synthesis of, l, 115.
vol. of croton oil, l, 116.
Alcohol, iso-butyl, conversion of, l, 114.
Arsenic and arsenous acid ethers, *Crafts*, l, 10.
barium chlorate, prep. of, l, 111.
beech-wood tar creosote, l, 117.
bromhydric acid, prep. of, l, 113.
Chloral, secondary prods. of, l, 109.
conjugate bodies in inorganic, *Blomstrand*, ix, 110.
copper, vol. estim. of, *Weil*, l, 108.
Eliot and Storer's, iii, 420; vi, 130.
Frankland's, iii, 137.
G. Fownes's, noticed, viii, 448.
Handbook of, *Rolf* and *Gillett*, vii, 294.
Harcourt's, noticed, ix, 141.
hydrogenium-amalgam, *Loew*, l, 99.
Miller's, noticed, iv, 295.
nitric anhydrid, prep. of, l, 112.
of copper, *Hunt*, ix, 153.
of the Bessemer process, l, 432.
of the Farm and Sea, by *Nichols*, noticed, iv, 141.
physiological, notices in, *Barker*, vi, 233, 379.
properties of precip. iron, *Lenz*, l, 110.
Roscoe's, iii, 137.
selenium, properties of, l. 111.
sulphur trioxyd, *Schultz-Dellack*, l, 114.
tantalic acid in microlite, l, 95.
thallium, position of, *Rammelsberg*, l, 108.
uranium, recovery of, l, 113.
Cheyney, J. S., mode of showing vibrations, vi, 243.
Chicago Museum, director of, iii, 281.
tunnel excavations, iii, 75.
Child, G., production of organisms, i. 381.
China, coal formation of, *Newberry*, ii, 151.
explorations in, *Richthofen*, l, 149, 410.
delta plain of, *Pumpelly*, v, 219.
geology of *Pumpelly*. i, 145.
journey through, *Bickmore*, vi, 1.
notes on, *Martin*, vii. 98.
See also *Geology*.
Chladni plates, mode of showing the vibrations in, *Cheyney*, vi, 243.
Chloral, secondary products of. l, 109.
Chlorid, carbonylic, *Schützenberger*, vii, 423.
Chlorids corresponding to peroxyds, i, 107.
Chlorine in organic compounds, *Warren*, ii, 156.
Christy, Reliquiæ Aquitanicæ, ix, 144.
Chromium, mode of obtaining, vi, 131.
and iron compounds of, *Corbin*, viii, 346.
Chromites of magnesium, *Nicklès*, vii, 16.
Chronograph, proposed, *Young*, ii, 99.
Chronology, new period in, *French*, l, 172.
Church, on namaqualite, l. 271.
Clark, A., receives Rumford medal, ii, 136.
Clark, H. J., animality of sponges, ii, 320.
Anthophysa Müllerí, ii. 223.
mind in nature, noticed, i, 286, 418.
on Spongiæ ciliatæ, v, 418.
on Vorticellidian parasite of Hydra, noticed, ii, 134, 284.
polarity and polycephalism, ix, 69.
Clarke, F. W., atomic volumes of liquids, vii, 180.
of solid elements, vii, 308.
of solid compounds, l, 174.
mineral analysis, v, 173.
on cobalt and nickel, viii, 67.
separation of tin from arsenic, etc., ix, 48.
Clarke, W. B., Dinornis and saurian remains of Australia, ix, 273.
sedimentary formations of New South Wales, v, 334.
Clausius, R., mechanical theory of heat with its application to the steam-engine, noticed. iv, 437.
Cleve, Ammonia-chromium bases, ix, 251.
Clouds, formation of, *Tyndall*, viii, 268.
Coal formation of China, *Newberry*, ii, 151.
of Langeac, Haute Loire, l. 269.
Coal-tar, hydrocarbons in, *Berthelot*, iv, 418.
Coal, see GEOLOGY.
work on, by *Geinitz*, noticed, i, 285, 430.
Coan, T., eruptions in Hawaii, i. 424; iii, 264; vi, 106; vii, 89; ix, 269, 393.
Coast Survey, magnetic observations in Maine, *Bache*, ii, 141.
recent Gulf-stream soundings, *Mitchell*, iii, 69.
report, v, 143; vii, 447; l, 439.
Superintendent appointed, iii, 281.
Cobalt, nitrate of, *Erdmann*, iii, 248.
and nickel, separation of, ii, 254; *Clarke*, on, viii, 67.
Cobbold, on beef and pork as sources of Entozoa, i, 283.
Coleoptera, rhynchophorous, *Le Conte*, iv, 41.

Color from mixing blue and yellow, *Rood*, i, 369.
Colorado, chalk in. i, 401.
geological observations in, iv, 284.
Mauvaises Terres in, l, 292.
parks of, iv, 351.
Sivatherium in, *Leidy*, l, 270.
Coloration by discharges from inductorium. v, 391.
Collier, D. C., chalk in Colorado, i, 401.
Colton's Journal of Geography, v, 142.
Combustion, rapid, production of ozone in, l, 255.
under pressure, vi, 394.
Comet. a new, *Winnecke*, l, 104.
Biela's, division of. iv, 130.
connection with meteors, iv, 128.
discovered, iii, 279.
III, 1862, elements of, iv, 130.
II, 1867, iv, 130.
of 1812 and 1846, *Kirkwood*, viii, 255.
prizes for discovery of, viii, 279; ix, 442.
Connecticut Academy of Arts and Sciences, Transactions of, ii, 138; l 437.
Conrad, T. A., check list of Eocene, etc., noticed, iii, 363.
new Eocene groups, i, 96
divisions of Eocene, *Hilgard* on, ii, 68.
fossil shells of the upper Amazon, noticed, l, 424.
mixtures of Cretaceous and Eocene fossils, ix, 275.
North and South Carolina Tertiary, iii, 260.
notes on American fossiliferous strata, vii, 358.
reply on the Cretaceous of California, iv, 376.
Cook, G. H., Geology of New Jersey, noticed, vii, 429.
Geol. map of N. J., noticed, vii, 112. 279.
Cooke, J. P., atomic ratio, vii, 386.
aqueous lines of spectrum, i, 178.
certain lecture experiments, and a new form of Eudiometer, iv, 189.
Chemical philosophy by, noticed, vii, 435; l 150.
cryophilite, iii, 217.
danalite, ii, 73
determination of protoxyd of iron in silicates, iv, 347.
heat of friction, i, 116.
on some American chlorites, iv, 201.
Copal of Zanzibar, ix, 123.
Cope, E. D., Alleghany fishes, noticed, vii, 285.
Dinosaur in N. J. Cretaceous, ii, 425.
Elasmosaurus platyurus, l, 140, 268.
Cope, E. D., extinct Batrachia and Reptilia of N. A., Part II, noticed, l, 150.
Extinct cave-manmals of U. S., noticed, ix 273.
hypothesis of evolution, l, 432.
Maack's Schildkröten, noticed by, l, 136.
Megadactylus polyzelus, ix, 390.
new Enaliosaur, vi, 263.
on genus Lælaps, vi, 415.
on Raniform Aneura, noticed, v, 418.
on some reptilian remains, viii, 278.
synopsis of extinct Batrachia, Reptilia and Aves of N. A., viii, 450.
Copley medal to Chasles, i, 428.
Copper, chemistry of. *Hunt*, ix, 153.
diffusion of *Nicklès*, v, 67.
precipitation of, by hypophosphorous acid, *Gibbs*, iv, 210.
red oxyd of, *Blake*, iii, 124.
and nickel, precipitation of, by alkaline carbonates, iv, 213.
volumet. estimation of, *Weil*, l, 108.
Corals, *Verrill*, viii, 419, 431, 432; ix, 370.
Corbin, J. H. H., on compounds of chromium and iron, viii, 346.
Cordova observatory, Gould at, l, 144.
Cotta's Ore-deposits, noticed, ix, 271.
Cotting, J. R., obituary, v, 141.
Coues's lists of birds, noticed, vii, 150.
antero-posterior symmetry, etc., noticed, l. 432.
Cowan's facts in the history of insects, noticed, i, 135.
Cox, E. T., Indiana geol. survey, l, 135.
ethers of arsenic, l, 10.
Crafts, J. M., silicic acid ethers, iii, 155, 331.
silicon, combinations of, with alcohol-radicals, ix, 307.
qualitative analysis, ix, 142.
Craig's decimal system, noticed, iii, 423.
Creighton's tree-labels, iii, 273.
Cremona, work by, noticed, l, 438.
Creosote from beech-wood tar, l, 117.
Cretacous of California, subdivisions of, *Gabb*, iv, 226.
Conrad, in reply to Gabb, iv, 376.
of Iowa, exogenous leaves in, *White*, iv, 119.
plants of Nebraska, vi, 91.
of N. America, vi, 401.
Crinoidea, paleozoic, *Meek*, viii, 23; l, 135.
Billings, viii, 69; ix, 51; l, 225. 435.
convoluted plate in, *Hall*, i. 261.
Crocodile in Florida, *Wyman*, ix, 105.
Croll, J., ocean currents as distributers of heat, l, 118.
on motion of glaciers, viii, 273.
on Scottish glaciers. l, 292.
Cronise's wealth of California, noticed, vii, 150.

Crookes's edition of Mitchell on assaying, vi, 434.
metallurgy, noticed, ix, 144, 286.
Crotonic acid, iii, 110.
Croton oil, volatile acids of, l, 116.
Crowther, B., petroleum in Mexico, vi, 147.
Crustacea of Brazil, *Smith*, viii, 388.
localities of Artemia, viii. 430.
new Phyllopod, *Verrill*, viii, 244.
Crustacean parasitism, *Verrill*, iv, 126.
Crystals, expansion of, iii. 255.
on measuring, *Blake*, i, 308.
Crystallographic and crystallogenic contributions, No. IV, *Dana*, iv, 89, 252, 398.
Currents, marine, of the north circumpolar basin, iv, 383.
Curtis, Botany of N. C. survey, v, 271.
Esculent Fungi of United States, noticed, ii, 129.
Cyanic ethers, iii. 388.
Cyanhydric acid, *Berthelot*, vii, 420.
homologues of, *Hofmann*, iv, 416.
Cyclopedia, American, noticed, ii, 138; iv, 140; vi. 431.

D

D'Agelet's observations on fixed stars, noticed, iv, 287.
Dall, W. H., explorations in Russian America, i, 139; v, 96.
on a pure scarlet. l, 291.
Dana, J. D., on cephalization, with replies to objections, i, 163.
on spontaneous generation, i, 389.
on Shepard's corundophilite and paracolumbite, ii, 269.
on turnerite, ii, 420.
origin of earth's features, ii, 205, 252.
connection between crystalline form and chem. constitution, iv, 89, 252.
crystallograpic and crystallogenic contributions, No. IV, iv, 89, 252, 398.
on mineralogical nomenclature, iv, 145, 436.
chemical formulas of the silicates, iv, 252. 398.
chemical formulas of the feldspars, micas, scapolites, etc., and relations between proportion of silica and alkalies, iv, 398.
isomorphism of isometric and clinohedral feldspars, iv, 406.
classification of the elements, iv, 261.
editorial note in answer to Hinrichs, v, 102.
note on anhydrous silicates, v, 109.
Hawaii, eruption in. vi, 105, 122.
on the magnetite in the Pennsville mica, viii, 360.
Dana, J. D., geol. of the New Haven region, etc., noticed, ix, 275.
Mineralogy of, 5th ed., noticed vi, 132.
corrections for, viii, 433.
Dana, S. L., obituary of, v, 424.
Darwin on climbing plants, noticed, i, 125.
variation of animals under domestication. noticed. v, 141; *Brewer*, vi, 140.
Daubeny, C., obituary, v, 124, 272.
ozone from plants, iii, 273.
Daubrée on meteorites, ii, 124.
Substances Minerales, etc., noticed, viii, 419.
synthetic experiments on meteorites, noticed, viii, 419.
Davidson, G., effects of sun's heat on a sand-hill, ix, 255.
Dawson, J. W., Acadian Geology, noticed, vi, 267.
Eozoon Canadense, iii, 270; vi, 245.
fossils from the Laurentian, and on Eozoon, iv, 367.
graphite in Canada Laurentian, l, 130.
new Zonites, vi, 265.
paleozoic insects, iv, 116.
Day, Jeremiah, obituary of, iv, 291.
Dead Sea, formation of, *Lartet*, ii, 266.
Debray, on chlorids of tungsten, ii, 254.
on compounds of molybdic and phosphoric acids, vi, 397.
De Candolle, A., Address at Botanical Congress, ii, 129, 230.
laws of nomenclature at Botanical Congress, vi, 63.
Botanical nomenclature, l, 274.
Prodromus, noticed, ii, 427; vi, 408.
De Candolle, C., Piperaceæ, noticed, iii, 128.
theorie de la feuille, vi, 272.
De Forest, E. L., on correcting an error of temperature from unequal lengths of months, i, 371.
on correcting monthly means, ii, 154, 289.
on reducing meteorological observations, iii, 316.
De la Rue, W, solar physics, iii, 179, 322.
Delesse, Carte Agronomique, noticed, i, 429.
Hydrographic map of the department of the Seine, noticed, iii, 135; iv, 143.
Le Metamorphism des Roches, noticed. viii, 138.
Lithologie des Mers, ix, 144, 286.
on Origin of rocks, noticed. ii, 440.
Revue de Geologie, etc., noticed, i, 286.
Delessert F., obituary of. vii, 142.
Density, finding difference of. iii, 390.
Des Cloizeaux, action of heat on crystals, iv, 112.
on pachnolite, iii, 271.
on transparent wolfram, viii, 137.

Des Cloizeaux on wolframite, l, 271.
Descloizite of Wheatley mine, *Smith*, viii, 137.
Desor, on Sahara, noticed, i, 143.
Deville, graphitoidal boron, iii, 250, 388.
on magnesia in hydraulic cements, i, 425.
Dewey, C., Caricography, i, 226, 326; ii, 243, 325.
obituary of, v, 122.
Dexter, W. P., chemical apparatus, vi, 51.
sulphates of antimony, vi, 78.
preparation of hydrofluoric acid, ii, 110.
Diamond, colored by heat, ii, 270.
origin of, *Chancourtois*, ii, 271.
in Australia, ix, 275.
in Oregon, viii, 441.
Diamylene, oxydation with chromic acid, *Walz*, v, 57.
Diatomaceæ, spectroscopic examination of, viii, 83.
Dickerson, A. B., volcanic eruption, v, 131.
Dissociation of ammoniacal compounds. *Isambert*, ix, 387.
Distillation, on products of, *Assmuss*, v, 274.
Dodo, skeletons of, discovered, i, 273.
Doherty, Organic Philosophy, vol. II, v, 427.
Dolomite, formation of, *Hunt*, ii, 63.
gypsum decomposed by, *Hunt*, ii, 60.
Douglas, J., source of muscular power, v, 110.
Downing, A. J., fruits, etc., of America, noticed, ix, 142.
Dredging, ocean, ix, 129, 410.
Drift-boulder and scratches of Englewood, *Dwight*, i, 10.
Drift in Labrador, *Packard*, i, 30.
in Wisconsin, *Bliss*, i, 255.
in South Wales, *Symonds*, i, 259.
in western and southern states, *Hilgard*, ii, 343.
in southwest Iowa, *White*, iv, 119.
western. *Andrews*, viii, 172.
Dudley Observatory annals, noticed, ii, 139.
Dupont, Belgian bone-cave, iii, 121, 260.
Durangite, *G. J. Brush*, viii, 179.
Dutton, C. E., chemistry of the Bessemer process, l, 432.
Dwight, W. B., boulder of Englewood i, 10.
Coxsackie land sinking, i, 12.
Dyeing, Schützenberger on, noticed, iii, 421.
Dynamo-magnetic machine, *Ladd*, v, 115.

E

Earth, origin of features of, *Dana*, ii, 205, 252.
Earthquake in Australia, vi, 427.
in Bogota, *S. A. Hurlbut*, l, 408.
in California, 1868, vi, 428.
in New England, viii, 418.
in Kansas, vi, 132; *Parker*, v, 129.
in N. York, Vt., and Canada, v, 135.
in South America, 1868, vi, 422.
in St. Thomas v, 133.
in New England, Oct., 1870, l, 434.
Earthquakes, library of works on, for sale, ii, 290.
Perry on, noticed, v, 268.
Winslow on, ii, 45.
Earthquake waves, etc., *Coan*, ix, 269.
Earth's crust, change of axis of, *Evans*, iii, 230.
Earth's orbit, secular variation in elements of, *Stockwell*, vi, 87, 436; l, 147.
Eastman, J. R., height of Kearsarge mountain, viii 439.
Eaton, D. C., Gray's field, forest, and garden Botany, noticed by, vii, 280.
Hooker's Synopsis Filicum, vii, 143.
Botanical notices, l, 425.
Eclipse, solar, on observing corona during, *Newcomb*, vii, 413.
spectroscope observations, vii, 127.
See further, *Sun*.
Edlund, galvanic expansion, iii, 256.
Edwards, A. M., living forms in hot waters of California, v, 239.
Edwards's Oiseaux fossiles de la France, noticed, vii, 276.
Edwards, W. H., Butterflies of North America by, noticed, vi, 150, 436; viii, 144, 433; ix, 427.
Eggertz, on determining carbon in iron, vii, 374.
Eggs, coloring matter of, *Städeler*, vi, 235.
Egleston, T., geological survey, noticed, iii, 114.
Elam's Physicians Problems, ix, 444.
Elasmognathus, *Gill* iii, 370.
Elasmosaurus, note on, *Cope*, l, 140, 268.
Elba stone implements, i, 427.
Elderhorst, W., Blowpipe analysis, iii, 136.
Electrical machine, form of discharge between poles of, *Wright*, ix, 381.
Electric apparatus, new, iii, 107, 247, 386.
brush and glow, spectrum of, iii, 394.
conductivities, method of measuring, *Mayer*, l, 307.
currents by percussion, *Rood*, ii, 12.
light, cost of, *Farmer* v, 112.
spark, figures produced by, ix 289.
telegraph, Manual of, by F. L. Pope, noticed, viii, 150.
Electricity, discharge of Leyden jar with induction coil, *Rood*, viii, 153.
Faraday's discoveries in, *Tyndall*, vi, 34, 180.
Electrification of an island, l, 148.

Electro-magnetism, researches in, *Mayer*, l, 195.
Electro-motive force, *Haug*, ii, 381; iii, 43.
Elements, atomic volumes of, vii. 180, 308.
new, associated with zirconium, viii, 405.
spectra of, *Hinrichs*, ii, 350.
Elevations, see HEIGHTS.
Eliot & Storer's Chemistry, noticed, iii, 420; vi, 130.
Elliot's birds of N. America, i, 433.
Encke, J. F., biographical sketch of, iii, 10.
Encyclopedia. Chamber's noticed, iv, 144.
Engel, C. G. F., obituary, v, 282.
Engelmann, G., Junci of N. America, ii, 128.
Engineering Magazine, Van Nostrand's, vii, 295.
Entomological correspondence of Harris, noticed, viii, 143.
Society, Philad., Transactions. i, 144; iv, 296; v, 144, 428; vi, 150, 151, 288.
Entomologist, American, noticed, vi, 435; l. 439.
Entozoa, beef and pork as sources of, i, 283.
new species of, *Verrill*, l, 223.
Eocene, division of. *Conrad*, i, 96; *Hilgard*, ii, 68.
See also GEOLOGY.
Eozoön, *Carpenter* on, i, 406; vi, 245.
in Finland, iv, 284.
in Massachusetts. ix. 75.
organic nature of, *Dawson* and *Carpenter*, iv, 367; inorganic nature of, *King* and *Rowney*, iv, 375.
See also GEOLOGY.
Erbia, *Bahr* and *Bunsen*, i, 399.
Erdmann, A. J., obituary of, ix, 144.
O. L., obituary of, ix, 144.
Erdmann's Exposé des formations Quaternaires de la Suede, noticed, vi, 435.
Essex Institute, Bulletin. vii, 436.
Peabody's gift to, iii, 416.
Proceedings, i, 144, 431; ii, 140, 292; iii, 424; iv, 296; v, 144, 428; vi, 288; vii, 296; ix, 444.
Ethers, arsenic and arsenious acid, *Crafts*, l, 10.
butyric and caproic, synthesis of, i, 115.
chlorinated, for synthesis of alcohols, *Lieben*, iii, 249.
cyanic, iii. 388.
of silicic acid, *Friedel* and *Crafts*, iii, 155, 331.
Ethyl. arseniate of, l, 10.
arsenite of, l, 15.
Ethylates, potassic and sodic, viii. 413.
Ethylene-sodium, a new radical, *Wanklyn*, viii, 413.
Eudiometer, a new form of, *Cooke*, iv, 199.
Evans, E. W., oil-bearing uplift of W. Virginia, ii, 334.
Evans, J., geological changes in position of axis of earth's crust. iii, 230.
Expansion, galvanic, iii, 256.
of crystals, iii, 255.
of metals and alloys by heat, *Matthiessen*, iv, 110.
of solids, measure of, *Müller*, viii, 407.
of water and mercury, iii, 254.
Expedition, polar, new, i. 427.
Exploration in Russian America, connected with Telegraph Co., i, 139.
Exploring expedition to the interior basin, *King*, iv, 118.
Exposition, Paris. 1867. *Nicklès*, v, 73.
minerals of, *Blake*, v, 194.
Reports on. ix, 287; *Barnard's*, viii, 297; *Blake's*, viii, 126; *Smith's*, viii, 447; list of, ix, 258; l, 296.
Eye-piece, new binocular, *Smith*, v, 42.

F

Falling bodies, apparatus for showing laws of, ii, 418.
Faraday, obituary, iv, 293; *Tyndall*, vi, 34. 80; *de la Rive*, v. 145.
Farmer, M. G., cost of electric light, v, 112.
magnetic effects of the aurora, i, 118.
mechanical equivalent of light, i, 214.
Farmer's theorem discussed, *Stimpson*, l, 372; note on Stimpson's paper, *Silliman*, l, 377.
Farquhar, E. J., new variable star, ii, 79.
Fay, T. S., Great Outline of Geography by, noticed, l, 151.
Featherstonhaugh, G. W., obituary of, iii, 135.
Femur, work on excisions of, for gunshot injuries, noticed, viii. 448.
Fendler, A, on prairies, i, 154.
Ferns, arrangement of, *Hincks*, l. 276.
Ferrocyanid of potassium, action of on monochloracetic ether, *Loew*, v, 383.
Feuchtwanger's treatise on Gems, noticed, iv, 295.
Field's History of Atlantic Telegraph, noticed, ii, 437.
Finlay, G., prehistoric Archæology in Greece, l, 251.
Fischer's salt, *Sadtler*, ix, 189.
Fishes destroyed in Bay of Fundy, vi, 269.
Fizeau, expansion of crystals, iii, 255.
Flagg's Hand-book of the Sulphur-cure for the vine-disease, etc., noticed, ix, 442.
Flame, Bunsen, theory of, *Knapp*, l, 255.
monochromatic, *Nickles*, iii, 92, 93.
reactions, *Bunsen*. iii. 110.
sodium, *Nicklès*, iii, 92.
sounding, *Smith*, v, 421, 422.

Flames, luminous, *Hilgard*, vii, 218.
Flammarion's Astron. studies, iii, 423.
Fleck, H., on sodium in explosives, vii, 431.
Florida, change of level on coast of, i, 406.
Fluorescence, *Loughlin*, iii, 239.
Fluohydric acid, properties of, *Gore*, viii, 406.
Fluorids, new, *Nicklès*, v, 66.
Flying-fish. *H. Mann*, i, 272.
Fonda, on Franklinite, viii. 138.
Footmarks in Kansas, *Mudge*. i, 174.
Foraminifera in depths of ocean, ix, 415.
Forbes, J. D., obituary, vii, 294.
Force and Will, *Gould*, ix, 277.
Formic versus carbonous acid, *Barker*, iv, 263.
Fossil, see GEOLOGY and ZOOLOGY.
Foster, J. W., Mississippi Valley by, noticed, viii. 150.
Foucault's objectives, iii, 254.
Fournier on Cruciferæ, noticed. ii, 277.
Fownes, G., Chemistry by, noticed, viii, 448.
Frankland, E., acids of the lactic series, iv, 268.
Chemistry by, noticed, iii, 137.
on combustion under pressure, vi, 394.
on gaseous spectra, viii, 402.
source of muscular power, ii, 393.
Fraunhofer's lines, wave-lengths of, i, 395.
French, J. W., precession-period in Chronology, l, 172.
Fresenius, Johnson's edition of. ix, 255.
Fresnel's theory of reflexion from glass surfaces, *Rood*, iii, 104.
Friction. heat of. *Cooke*, i, 116.
Friedel, C., silicic-acid ethers. iii, 155, 331.
on silico-propionic acid, l, 416.

G

Gabb, W. M., Cretaceous of California, iv, 226.
Paleontology of California, noticed, i, 284; v, 288; viii. 133.
Gaffield, T., action of sunlight on glass, iv, 244, 316.
Gallatin, A. H., on ammonium and on tests for nascent hydrogen, ix, 256 257.
Galvanic battery, *Hough*, viii, 182.
circuit, resistance of, *Haug*, ii, 383; iii, 43.
Gas, effect of mixture with atmospheric air on illuminating power of, *Silliman* and *Wurtz*, viii. 40.
lamp and regulator, *Dexter*, vi, 51.
on determining the photometric power of, etc., *Silliman*, l, 379.
relation of light from, to volume consumed, *Silliman*. ix, 17.
well in New York, *Wurtz*, ix, 336.
Gases, specific gravity of. *Schmidt*, iii, 391.
Gasparini, G., obituary, v. 124.
Gasteropods, sulphuric acid of, ix, 420.
Gaussoin, E., on island of Navassa, noticed, ii, 439.
Geinitz. H. B., plants of the coal formation of Langeac, Haute Loire, l. 269.
on a German meteorite, vi, 284.
on Taconic slate in Germany, iii, 409.
Steinkohlen Deutschlands, etc., noticed. i, 285, 430.
on the upper Paleozoic rocks and fossils of S. E. Nebraska, review of, *Meek*, iv, 170. 282, 327.
Gems, Feuchtwanger's treatise on, noticed, iv, 295.
Genth, F. A., cabinet for sale, i, 141.
chrysolite with chrome iron, in Penn.. i, 120.
mineralogical contributions v, 305.
Geographical distribution of birds, i, 78, 184, 337.
notes on earliest discoveries in America, *Stevens*, viii, 299, 437.
notices, *Gilman*. iv, 377; vii, 98, 377.
Soc. of London, Journal, noticed, iv, 381.
Geographie ancienne et moderne, dictionnaire de, noticed, iv, 382.
Geographisches Jahrbuch, 1866, noticed, iv, 382.
Geography, Great Outline of, noticed, l, 151.
Geological cabinet, *Genth's*, for sale, i, 141.
Sketches, by *Agassiz*, noticed, i, 407.
Charts. l, 425.
Reports, list of American, iii, 116, 399.
GEOLOGICAL WORKS, noticed:—
Barrande, Ptéropodes Siluriens de la Bohéme. v, 120.
Bigsby, Thesaurus siluricus, vi, 435.
Billings, fossils of Anticosti, iii, 137, 259.
fossils of Canada, i, 124.
Buckley. on Texas Geol. survey, ii, 438.
Catlin, Rocks of America, l, 135.
Cocchi, central Geology of Italy. ii, 123.
Cook, Geology of New Jersey. vii, 112, 279, 429.
Dana, geology of the New Haven region, etc., ix, 275.
Dawson, Acadian geol., new ed., vi, 267.
Delesse, Carte geologique du department de la Seine, ii, 440, iii, 135.
Origin of rocks, ii, 440.
Revue de Geologie, i, 286; iv, 122; vi, 270.
Edwards, Oiseaux fossiles de la France, vii, 276.
Egleston, survey west of Omaha, iii, 114.
Erdmann, Exposé des Formations quaternaires de la Suède, vi, 405.

GEOLOGICAL WORKS noticed:—
Erdmann, map of Sweden, i, 429.
Gabb, Paleontology of California, i, 284; v, 288; viii, 133.
Gaussoin, Island of Navassa, West Indies, ii, 439.
Geinitz, see GEINITZ.
Geological Magazine, ii, 273.
Hall, J., new Crinoidea, iii, 409.
Paleaster, etc., iii, 409.
Paleontology of New York, iv, 142, 273.
Hamilton, Report on Nova Scotia mines, ii, 123.
Hayden, Report of Colorado, ix, 258.
the Yellowstone and Missouri, ix, 118.
Rocky Mountain scenery, l, 125.
Hitchcock, maps in preparation, vi, 140.
Geol. Rep. on N. Hampshire, l, 425.
Houghton, Manual of Geology, iv, 121.
How, survey of Nova Scotia, ii, 123.
Hyatt, fossil cephalopods, v, 419.
Julien, Geology of Sombrero, ii, 439.
Kerr, Report on N. C. survey, iii, 284.
Kjerulf and *Dahl*, chart of Norway, vi, 269.
Lapham, map of Wisconsin, vii, 279.
Lartet and *Christy*, Reliquiæ aquitanicæ, ii, 291; vi, 151, 279.
Lesquereux, Fucoids in coal-formation, ii, 264.
Tertiary plants of Mississippi, iv, 121.
Logan, Canada survey, iii, 254.
map and sections by, i, 408.
map of Canada, ix, 394.
Maack's Schildkröten, *Cope*, l, 136.
Matthew, rocks of S. New Brunswick, ii, 124.
Meek, Bellerophontidæ, ii, 126.
crinoids, anat. of, l, 135.
geology of Mackenzie river, v, 418.
on fossils of Illinois, etc., and on Spirifer cuspidatus, i, 409.
and *Worthen*, Illinois paleontology, iii, 111.
Meneghini, Dentex Münsteri, ii, 124.
Moore, fossils in mineral veins, l, 265.
Mudge, Rep. Geol. Kansas, iii, 283.
Murchison, Siluria, v, 121.
Newberry, Chinese coal fossils, iii, 284.
survey of Ohio, ix, 400.
Packard's glacial phenomena of Labrador and Maine, iv, 117.
Pumpelly, China, Japan, etc., iii, 408.
Ramsay, geology of N. Wales, ii, 265.
Rémond, geology of N. Mexico, ii, 261.
Roemer, spider from coal-formation, ii, 123.
Safford, Tenn. Geol. Report, i, 409; vi, 435; viii, 416.
GEOLOGICAL WORKS noticed:—
Safford, geol. map of Tenn., i, 285.
Seeley, Ornithosauria, l, 134.
Shumard, Cretaceous of Texas, ii, 123.
paleozoic fossils, i, 124; ii, 118.
Simonda, anthracite rocks of Alps, iii, 409.
chart of N. W. Italy, iii, 115.
Smithsonian contributions to paleontology, iii, 363.
Swallow, Rep. Geol. Surv. Kansas, iii, 283.
Vose, Orographic geology, ii, 123, 205.
White, Iowa survey Rep., iii, 284; iv, 121; v, 402.
Whitney, Rep. on Geol. of California, noticed and reviewed, i, 124, 231, 351.
Whittlesey, explorations in Minnesota, ii, 440.
Winchell, Grand Traverse region, ii, 268.
Woodward's British fossil Crustacea, iv, 116.
Worthen, Rep. Geol. Illinois, ii, 291; iii, 110, 395; vii, 151.
Winkler, Musée Teyler catalogue, v, 121.
Geological survey of California, i, 428; viii, 151; ix, 400.
Canada, iii, 254; ix, 394.
Colorado, iv, 284.
Indiana, viii, 138; l, 135.
Illinois, ii, 291; iii, 110, 395; vii. 151.
Iowa, ii, 272; l, 136.
Kansas, iv, 32.
Louisiana, viii, 331.
Nebraska iv, 284; v, 268.
New York, iv, 142, 273.
Nova Scotia, i, 285.
Ohio, viii, 417.
Tennessee, *Safford*, i, 285, 409; vi, 435; viii, 416.
GEOLOGY AND PALEONTOLOGY:—
Æpyornis, ix. 275.
Amazon fossils, l, 294, 424.
Amer. fossiliferous strata, *Conrad*, vii, 358.
Amiens gravel, *Tylor*, vi, 302, 436.
Andes of Ecuador, *Orton*, vii, 242.
Arizona, *Silliman* on, i, 289.
Australian gigantic marsupials, i, 258.
Aviculopecten, *Meek*, v, 64.
"Bad Lands" of Upper Missouri, *Hayden*, ii, 425.
Basin of the Great Lakes, *Newberry*, ix, 101.
Batrachia and Reptilia, *Cope*, l, 150.
Bellinurus Danæ, *Meek*, iii, 257, 394.
Blastoidea, structure of, *Billings*, vii, 353; l, 225.
Bone-caves, Brazil, *Reinhardt*, vi, 264.
Brazil, reptilian remains of, *Marsh*, vii, 390.

GEOLOGY AND PALEONTOLOGY:—
Burlington limestone formations, ii, 95.
Calamiteæ and Equisetaceæ, *Carruthers*, vii, 279.
California gold-fields, *Phillips*, vii, 134.
new facts in geology of, *Whitney*, i, 252.
Cave mammals, *Cope*, ix, 273.
Cephalaspis, species of, i, 261.
China and Japan, changes in, *Bickmore*, v, 209.
coal formation of, *Newberry*, ii, 151.
Japan, etc., *Pumpelly*, i. 145.
Coal in Nebraska, *Hayden*, v, 198, 326; *White*, v, 399.
in Rocky Mts., *Hayden*, v, 101.
"Cone in cone," *Marsh*, v, 218; *White*, v, 401.
Conn. River sandstone, supposed tadpole nests in, *Shepard*, iii, 99.
Corals from Nevada silurian, v, 62.
Crinoidea, *Meek* and *Worthen*, i, 124; viii, 23.
Hall, i, 261.
Cystidea, Blastoidea, *Billings*, viii, 69; ix, 51; l, 225.
Crab, oldest known British, ii, 264.
Cretaceous coal in New Mexico, *LeConte*, v, 136.
plants in Nebraska, vi. 91.
in N. America, vi, 401.
birds of, ix, 205, 272.
and Eocene, mixtures of, *Conrad*, ix, 275.
Dakota Cretaceous, *Hayden*, iii, 171.
Northeastern, geology of, *Hayden*, iii, 171.
Mammals of, ix, 274.
Dead Sea, *Lartet*, ii, 266.
Denudation, subaërial, *Whitaker*, vi, 268.
Dinornis, etc., in Australia, ix, 273.
Dinosaur, in Cretaceous of N. J., *Cope*, ii, 425.
Discosaurus, *Leidy*, l, 139.
Drift, absence of, from Pacific slope of Rocky Mts., *Brown*, l, 318.
in S. W. Iowa, *White*, iii, 301.
Earth's features, origin of, *Dana*, ii, 205, 252.
Elasmognathus, *Gill*, iii, 370.
Elasmosaurus platyurus, ix, 392; l, 140, 268.
Enaliosaur, new, *Cope*, vi, 263.
Entomostraca, Paleozoic, vi, 265.
Eocene, Conrad's group of, *Hilgard*, ii, 68.
new group in, *Conrad*, i, 96.
Eozoön, *Carpenter*, i, 406.
Bavaricum, iii. 398.
Canadense, *Dawson* and *Carpenter*, vi, 245, 436.
described by Dawson, iii, 270.
GEOLOGY AND PALEONTOLOGY:—
Eozoön in Massachusetts, ix, 75.
Euproöps, *Meek*, iii, 257, 394.
Fish-remains in Western N. Y., *Bradley*, ii, 70.
Fossils in California auriferous rocks, iii, 270.
Gavial from N. Jersey Eocene, *Marsh*, l, 97.
from China, *Richthofen*, l, 410.
Gibraltar caves, report on, vii, 277.
Glacial drift beneath Lake Michigan, *Andrews*, iii. 75; *Hilgard*, iii, 241.
Glaciers, cause of descent of, *Moseley*, l, 263.
in Green Mts., *Hungerford*, v, 1.
White Mts., iii. 42.
Gold-bearing rocks of California, *Blake*, v, 264; *Brewer*, ii, 114; v, 397.
Horse, tooth of, from Table Mt., *Blake*, l, 262.
Hudson river group, name of, *Meek*, iii, 256.
Illinois, new fossils in coal-formation of, *Meek* and *Worthen*, i, 123.
Niagara fossils near Chicago, *Winchell* and *Marcy*, i, 409.
Section of rocks of, *Worthen*, iii, 258.
Insects, in N. Scotia, etc., iv, 116.
Iowa coal measures, *White*, v, 331.
Kansas, Liassic footmarks in, *Mudge*, i, 174; *Swallow*, i, 405.
supposed footmarks in, *Hitchcock*, vii, 132.
Lakes of North America, sources of, iii, 193.
as Chronometers, *Andrews*, l, 264.
Laurentian of Canada, graphite in, l, 130.
of Nova Scotia, ix, 347; l, 132; 417.
fossils, *Dawson*, iv, 367.
Lias and oolite of Australia, *Moore*, l, 269.
Lignites of the West, *Hayden*, v, 101, 198.
Louisiana, lower, geol. of, *Hilgard*, vii, 77.
Maine, post-tertiary fossils of, ii, 426.
Mastodon in California, *Silliman*, v, 378.
remains, Cohoes, N. Y., ii, 426; iii, 115.
"Mauvaises Terres" in Colorado, l, 292.
Medusæ fossils, *Haeckel* on, ii. 133.
Megadactylus polyzelus of Hitchcock, *Cope*, ix, 390.
Megalonyx Jeffersoni in Ill., *Leidy*, l, 270.
Michigan, Marshall group of fossils, i, 120.

GEOLOGY AND PALEONTOLOGY:—
Mississippi, Quarternary of, *Hilgard*, i, 311.
Mosasauroid reptiles, *Marsh*, viii, 392.
Naiades, shell-structure of, *White*, v, 400.
Nevada fossils, *Meek*, l, 422.
New England. Geology of, *Hunt*, l, 83.
New South Wales, sedimentary formations, v, 334.
New York, fossils in Livingston and Genesee Cos., i, 121; ii, 426.
Oil-wells. position of, *Lesley*, i, 139.
Ontario, geology of, *Hunt*, vi, 355.
Pacific coast, gold rocks of, *Brewer*, ii, 114.
Palæotrochis of Emmons, *Marsh*, v, 217.
Paleozoic crustacea and cirripeds, ii, 272.
fossils. *Shumard's* list of, ii, 118.
Peat-bed underlying drift, *Orton*, l, 54.
Pipestone quarry, *Hayden*, iii. 15.
Plants of coal-formation, *Geinitz*, l, 269.
of Miocene, polar, v, 281.
of Cretaceous and Tertiary, N. A., vi, 401.
Post-tertiary of Montana, elephantine tooth and tusk in. i, 427.
Primordial, Lingula flags, Wales, i, 262.
Rocky Mts., formations along east margins of, *Hayden*, v, 322.
Secondary fossils in Oregon, *Blake*, iv, 118.
Serpent, new fossil, *Marsh*, viii, 397.
Silurian, Nevada, *Whitney*, iii, 267.
Sivatherium in Colorado, *Leidy*, l, 270.
South American notes, *Rémond*. iii, 114.
South Carolina phosphates, *C. U. Shepard, Jr.*, vii, 354.
origin of, *C. U. Shepard, Sr.*, vii, 338.
Sweden, bituminous gneiss and mica schist in, *Igelström*, v, 430.
Syringothyris, punctate, *Meek*, iii, 407.
Tapir, fossil, in California, *Blake*, v, 381.
Teterpeton Elginense, *Huxley*, iii, 406.
Tertiary, Mississippi and Alabama, *Hilgard*, iii, 29.
N. and S. Carolina, iii, 260.
Texas and Chihuahua, *Kimball*, viii, 378.
Vermont, geol. notes on, *Hunt*, vi, 222.
western, geol. of, *Perry*, vii, 341.
Volcanic, see *Volcano*.
Zonites, new, *Dawson*, vi, 265.
See also *Drift*, *Fossils*, *Man*, *Petroleum*, *Stone-implements*.
Geology and Revelation, *Molloy*, noticed, l, 151, 440.
Geometrical problems *Warren's*. iii, 284.
Geometry, Treatise on Elementary, *Chauvenet*, noticed, l, 438.
Geometry, *Cremona*, l, 438.
Germination influenced by various bodies, *Lea*, iii, 197.
Genther, volatile acids of croton oil, l, 116.
Gibbes, R. W., obituary of, ii, 435.
Gibbes, L. R.. on the occultator, vii, 191.
Gibbs, W., chemical and physical abstracts, i, 111, 250, 395; ii, 254; iii, 107, 246, 386; iv, 101, 265, 416; v, 249, 391; vi, 124, 258, 394; vii, 127, 272, 415; viii, 121, 401; ix, 106, 251, 386; l, 105, 413.
on map of the spectrum, iii, 1.
on a method of volumetric analysis, iv, 207.
theory of atomicities, iv, 209.
precipitation of copper by nitro-phosphorous acid, iv, 210.
precipitation of copper and nickel by alkaline carbonates, iv. 213.
employment of sand and glass filters in analysis, iv. 215.
estimation of manganese as a pyrophosphate, iv, 216.
obituary of Engel, v, 282.
measurement of wave-lengths, v, 298.
uric acid, vi, 289.
wave-lengths of spectral lines, vii, 194.
on action of alkaline nitrites upon uric acid and its derivatives, viii, 215.
method of avoiding observations of temperature and pressure in gas analyses, ix, 376.
Sprengel's pump in analysis, ix, 378.
optical notices, l, 45.
Gibraltar bone-caves. vii, 277.
Gill, T., Elasmognathus, iii, 370.
new species of tapir. l, 141.
Smithsonian contribution to paleontology, iii, 363.
Gilliss's astronomical observations, iii, 413.
Gilman, D. C., geographical notices, vii, 98. 377.
Gilman, W. S. Jr., auroras in Maine, vi, 390.
on aurora of April, 1869. viii, 114.
Giraud, J. P., Jr., obituary of, l, 293.
Glacial climate, *Murphy*, ix, 115.
Glaciers, cause of motion of, *Croll*, viii, 273.
cause of the descent of, l, 263.
of Alaska. *Blake*, iv, 96.
of Scotland, *Croll*, l, 292.
See also *Drift*, *Geology*.
Glass, action of sunlight on, *Gaffield*, iv, 244, 316.
crystalline nature of, *Wetherill*, i, 16.
lead-thallium, iii, 254.

Glass, silvering on, *Barker*, iii, 252.
test for parallelism in, *Gibbs*, l, 53.
Glucose, test for, *Braun*, iii, 250.
Glycerin and its derivatives, *Buff*, v, 256.
Glynn, J., Water as applied to moving mills, etc., l, 440.
Godwin, B., repetition in analysis, l, 249.
Goessmann, C. A., chemistry of brines, iv, 77.
salt, ix, 78.
Onondaga mineral springs, ii, 211, 368.
Gold, crystallized mass, Cal., *Blake*, i, 120.
decrease in prodcction of, vii, 432.
in Rhinebeck, N. Y., vii, 139.
in Scotland, vii, 280.
new solvents for, iii, 95.
of Montana, i, 125.
of Victoria, *Smyth*, ix, 263.
ores, Kustel on, *Brush*, vi, 287, 432.
peculiar occurrence of, *Silliman*, v, 92.
rocks of California, i, 351; v, 264; *Brewer*, ii, 114; v, 397.
See also *California*.
Gold-mining districts, Grass valley, *Silliman*, iv, 236.
Phillips on, noticed, vi, 134.
Goldschmidt, H., obituary of, iii, 89.
Good Hope, cape of, astronomer at, l, 149.
Gould, A. A., obituary of, ii, 434.
Gould, B. A., on Cordova observatory, l, 144.
on force and will, ix, 277.
on new variable star, ii, 80.
on solar eclipse, viii, 434.
on transatlantic longitude, ix, 228.
statistics of American soldiers, noticed, viii, 149.
Graham, T., obituary of, ix, 144.
Grapevine, sulphur-cure for diseases of, *Flagg*, noticed, ix, 442.
Graphite in Laurentian of Canada, l, 130.
Graphitoidal silicon, crystallization of, iv, 108.
Gray, A., Address by, on presentation of Rumford medal to Prof. Treadwell, i, 97.
American heather, iii, 128.
Botanical necrology, v, 121, 272; vi, 273; vii, 140; ix, 129.
notabilia, ix, 120.
notices, i, 125, 410; ii, 126, 273, 427; iii, 125, 272, 409; iv, 122, 420; v, 121, 269, 402; vi, 270, 408; ix, 403; l, 42, 274.
obituary of W. J. Hooker, i, 1.
dimerous flower of Cypripedium candidum, ii, 195.
nomenclature of Müller, iii, 126.
Shortia galacifolia, v, 402.
Gray. A., remarks on laws of botanical nomenclature, vi, 74.
Greece, prehistoric archæology in, l, 251.
Green, H. A., fossils of Livingston and Genesee cos., N. Y., i, 121; ii, 426.
locality of fossils in N. Y., i, 121.
Green Mts., glacial action on, *Hungerford*, v, 1.
Greg on meteors, *Newton*, iii, 285.
Greville, R. R., obituary of, ii, 277.
Grisebach, catal. plant. Cubens., iii, 409.
Guanape I., and its minerals, *Shepard*, l, 273.
Guanidin, synthesis of, iii, 110.
Guatemala, new species of tapir, *Gill*, l, 141.
Guerin, T., delivery from conduit pipe, v, 191.
Gulf-stream fauna, *Pourtales*, vi, 409, 413.
on the, etc., *Petermann*, noticed, l, 295.
soundings, iii, 69; vii, 379.
Gümbel, Eozoön Bavaricum, iii, 398.
Gun-cotton, *Abel*, iv, 288.
Gunther's record of zoological literature, noticed, i, 287.
Guyot's geographies, v, 287.
Gynœcological Society, Boston, vii, 288.

H

Hæmatoidin, *Holm* on, vi, 233.
Hagemann, G., analysis of pachnolite, i, 119.
crystallized cryolite, ii, 268.
Greenland minerals, ii, 93.
Ivigtite, vii, 133.
Haidinger, W. R., meteor of Knyahinga, iv, 131.
on meteorites, viii, 280.
Hail in China, *Williams*, iii, 281.
Hail-storm of June 20, 1869, *Hovey*, l, 403.
Hall, E., Flora of Eastern Kansas, l, 29.
Hall, Asaph, on secular perturbation of planets, l, 370.
Hall, J., convoluted plate in crinoids, i, 261.
geological map of U. States, by, vi, 140.
notes on geol. of Minnesota, iv, 144.
on new Crinoidea, iii, 409.
on Paleaster, iii, 409.
paleontology of N. Y., by, noticed, iv, 142, 273; vi, 262.
shells of the Upper Helderberg, noticed, ix, 276.
Halliday, A. H., obituary of, l, 294.
Hamilton, W. R., biography of, ii, 293.
elements of quaternions, noticed, ii, 438.

Hammond, R., Indiana geol. survey, l, 135.
Hand, morphology of, *Wilder*, iv, 44.
Hansen on moon's constitution, *Newcomb*, vi, 376.
Harcourt, exercises in chemistry, ix, 141.
Harris, T. W., entomological correspondence of, noticed, viii, 143.
Harrison, J. P., heat to moon from sun, i, 277.
Harvey, W. H., obituary of, ii, 129, 273.
Hassall on urine, noticed, i, 422.
Haug, H., electro-motive force, and resistance of galvanic circuit, ii, 381; iii, 43.
Hawaii, eruptions in, *Coan*, iii, 264; vi, 105; vii, 89; ix, 393.
Hawley, C. E., mines of Almaden, Spain, v, 9.
quicksilver mines of Santa Barbara, Peru, v, 5.
Hayden, F. V., Dakota geology, iii, 15, 171.
formations of east margins of rocky mountains, v, 322.
geology of Kansas, iv, 32.
geological survey of Colorado, etc., ix, 258.
lignite deposits of the West, v, 198.
report on the Yellowstone and Missouri, noticed, ix, 118.
Rocky Mt. coal beds, v, 101.
Sun-pictures of Rocky Mt. Scenery, l, 125.
tour through "Bad Lands," noticed, ii, 425.
workable coal in Nebraska, v, 326.
Hayes, A. A., color of water of Lake Leman, ix, 186.
Hayes, E., obituary of, iv, 139.
Heat, absorbed by water vapor, ii, 259.
action of, on crystals, *Des Cloizeaux*, iv, 112.
atomic, *Schmidt*, iii, 391.
dew formed by, iii, 246.
distrib. by ocean currents, l, 118.
of, over the earth, *Chase*, iv, 68.
emission and absorption of, *Magnus*, ix, 106.
flame, use of, in laboratory, *Smith*, l, 341.
low, emission, absorption and reexion of, *Magnus*, l, 105.
mechanical theory of, with its application to the steam-engine, *Clausius*, noticed, iv, 437.
of combination of boron and silicon with chlorine and oxygen, ix, 386.
of friction, *Cooke*, i, 116.
of sun, effects on a sand-hill, ix, 255.
reflection of, from fluor-spar, etc., *Magnus*, ix, 107.
Heat, Magnus, to moon from sun, *Harrison*, i, 277.
undulatory theory of, *Babinet*, iv, 111.
Heer, Miocene polar flora, v, 281.
Heights, see *Altitudes*.
Heintz, on formula of urea, vi, 237.
recovery of uranium, l, 113.
Helmholtz, takes chair of Physics in Berlin, l, 293.
Henfrey's Elementary Botany, noticed, l, 429.
Hennessey, J. H. N., on distrib. of temperture, noticed, iv, 438.
rain-fall affected by moon, vi, 283.
Henry, J., extract from Smithsonian report on transfer of the library, v, 137.
Henry, T. H., nickel and cobalt from manganese, vii, 130.
Hercynite, *Wolle*, viii, 350.
Hermann, on some minerals, l, 270.
Hessenberg, min. notizen, noticed, i, 409; ix, 402.
Hetch-hetchy valley, *Hoffmann*, vi, 266.
Hilgard, E. W., drift in western and southern states, ii, 343.
Quaternary of Mississippi, i, 311.
geology of Louisiana, vii, 77.
Illinois glacial drift, iii, 241.
Miss. and Alab. Tertiary, iii, 29.
on Conrad's Eocene, ii, 68.
on geol. reconnoisance of Louisiana, viii, 331.
processes in luminous flames, vii, 218.
reclamation by, v, 278.
Hilgard, J. E., longitude measured by telegraph, iii, 130.
Hill, T., on the occultator, vi, 299.
Hillebrand, W., eruption in Hawaii, vi, 115.
Hind, H. T., Laurentian in Nova Scotia, ix, 347.
Hines, C. F., on meteors during eclipse, viii, 435.
Hines, H. K., ascent of Mt. Hood, iii, 416.
Hinrichs, G., physical abstracts, ii, 258, 417; iii, 252, 390; iv, 110.
spectral lines by, ii, 350.
answers to charges of, *Dana*, v, 102.
atom-mechanics, *Fleck*, vi, 258.
Hitchcock, C. H., N. A. geol. atlas by, vi, 140.
supposed fossil footmarks in Kansas, vii, 132.
Geol. Rep. on N. Hampshire, l, 425.
Hoffmann, C. F., Hetch-hetchy valley, vi, 266.
Hofmann, A. W., a new class of homologues of cyanhydric acid, iv, 416.
guanidin, iii, 110.
on methylic aldehyd, v, 249.
on persulphid of hydrogen, vi, 396.

Hofmann, A. W., on utilizing secondary products of chloral manufacture, l, 109.
Holm, on hæmatoidin, vi, 233.
supra-renal glands, vi, 236.
Honeyman, Laurentian of N. Scotia, 417.
Hood, Mt., in Oregon, ascent of, iv, 429.
Hooker, J. D., Flora of British Island, l, 281.
Hooker, W. J., obituary of, i, 1.
Synopsis Filicum, vii, 143.
Hoopes on evergreens, noticed, vi, 270.
Hörnes, M., obituary of, vii, 294.
Hough, G. W., automatic printing barometer, i, 43.
galvanic battery, viii, 182.
Hovey, H. C., hail storm in Mass., l, 403.
How, H., analyses of shells, i, 379.
silicoborocalcite and natroborocalcite, v, 117.
Huggins, W., on heat of stars, viii, 286; ix, 108.
spectrum of new star, ii, 389.
Human remains, see *Man*.
Humboldt, life of *Stevens*, ix, 1.
Hungerford, E., evidences of glacial action on Green Mts., v, 1.
Hunt, T. S., lime and magnesia salts, ii, 49.
objects and method of mineralogy, iii, 203.
metallurgical method of Whelpley and Storer, iii, 305.
geology of Vermont, vi, 222.
Ontario, vi, 355.
of E. New England, l, 83.
Laurentian rocks in Mass., ix, 75.
chemistry of copper, ix, 153.
on norite, ix, 180.
labradorite rocks at Marblehead, ix, 398.
seat of volcanic action, l, 21.
Laurentian of Nova Scotia, l, 132.
Hurlbut, S. A., earthquake at Bogota, l, 408.
Hurricane, Bahamas, 1866, *Redfield* on, vii, 116.
Huxley, T. H., Address of, to the British Association, l, 383.
Telerpeton Elginense, iii, 406.
Hyatt, A., Fossil Cephalopods, noticed, vi, 150.
on Polyzoa, noticed, vi, 150.
parallelism between the stages of life in the tetrabranchiates, iv, 124.
Hydracids, action of, upon ethers, *Gal*, iv, 103.
Hydrocarbons, *Berthelot*, iv, 266, 418.
boiling point, etc., of, vii, 424.
distillation of, *Peckham*, vii, 9.
from animal fats, iii, 250.
new, *Schorlemmer*, iii, 108.
of Pennsylvania petroleum, v, 262.
Hydrocarbons, synthesis of, *Berthelot*, iii, 96, 251, 386, 389.
Hydrofluoric acid, preparation of, ii, 110.
Hydrogen and palladium, *Graham*, on, vii, 417.
nascent, tests for, ix, 256, 257.
persulphid of, *Hofmann*, vi, 396.
saturation with, *Berthelot*, vi, 125, 395.
Hydrogenium-amalgam, *Loew*, l, 99.
observations on, viii, 405.
Hydroxylamine, *Lossen*, i, 251.
synthesis of, *Lossen*, ix, 254.
Hypononce Sarsii, *S. Lovén*, viii, 429.
Hyposulphites, new test for, *Lea*, iv, 222.

I

Ice, artificial, viii, 440.
Iceland, revision of flora of, *Babington*, noticed, l, 277.
Igelström, L. I., bituminous gneiss in Sweden, v, 38.
Illinois, geol. survey of, noticed, ii, 291.
Megalonyx Jeffersoni in, *Leidy*, l, 270.
Illuminating power of gas mixed with atmospheric air, *Silliman* and *Wurtz*, viii, 40.
Indian summer, *Willet*, iv, 340.
Indiana, geol. survey of, *Cox*, viii, 138; l, 135.
Indium, iv, 110.
Insects, Packard's guide to the study of, noticed, ix, 285.
Paleozoic, in Nova Scotia and New Brunswick, *Dawson*, iv, 116.
Iodhydric acid, iii, 109.
preparation of, viii, 411.
Iodid of silver, light on, *Lea*, ii, 198.
Iodine, detection of, *Lea*, ii, 109.
in lead disease, etc., i, 110.
Iowa, geology of, *White*, iv, 23.
geol. survey of, ii, 272; l, 136.
Iron, cast, permeable to gases, v, 392.
magnetic oxyd of, Chester, Mass., ii, 93.
native hydrates of, *Brush*, iv, 219.
ore, specular, *Blake*, iii, 125.
properties of precipitated, *Lenz*, l, 110.
separation of sesquioxyd of, ii, 78.
specific magnetism of, *Chase*, v, 247.
Irons, meteoric, see *Meteoric*.
Isambert, dissociation of ammoniacal compounds, ix, 387.
Isomerism, *Berthelot*, ii, 257.
Isomorphism of leucite and other feldspars, *Dana*, iv, 406.
Itacolumite, *Wetherill*, iv, 61.
Ivory, artificial, i, 427.

J

Jackson, *C. T.*, Colorado meteorite, iii, 280.
and *J. C.*, analyses of minerals, Chester, Mass., ii, 107, 421.
Japanese alloys, *Pumpelly*, ii, 43.
Jenkin, *F.*, electrification of an island, l, 148.
Johnson, *S. W.*, assimilation of nitrogenous bodies by vegetation, i, 27.
edition of Fresenius, ix, 255.
Eliot and Storer's Chemistry, iii, 420; vi, 130.
How crops grow, by, noticed, vi, 436; vii, 147.
How crops feed, by, noticed, ix, 403.
kaolinite and pholerite, iii, 351, 405.
nitrification, vii, 234.
on estimation of carbonic acid, viii, 111.
on Peat, noticed, ii, 439.
terpin, iii, 200.
Jones, *J. M.*, on Nat. Hist. of Bermudas, Coralliaria, viii, 143.
Jones, *T. R.*, Foraminifera of oceans, noticed, i, 287.
Jukes, J. Beete, obituary of, viii, 296.
Julien's geology of Sombrero, ii, 439.

K

Kaemtz, head of Russian observatory, ii, 286.
Kansas, Eastern, Flora of, *Hall*, l, 29.
footprints in rocks of, i, 174.
geology of, *Hayden*, iv, 32.
Natural History Soc., l, 435.
notes on geology of, *Swallow*, i, 405.
Kearsarge Mt., altitude of, *Eastman*, viii, 439.
Kekulé, *A.*, Professor, iv, 137.
existence of chem. atoms, iv, 270.
Kellogg mines, Ark., *Smith*, iii, 67.
Kengott's Minerale der Schweiz, ii, 125.
Kennicott, R., in Russian America, i, 139.
obituary of, ii, 435.
Kent's cavern explorations, iii, 372.
Kentucky, petroleum in, *Safford*, ii, 104.
Kerr's survey of N. Carolina, noticed, iii, 284.
Kimball, *J. P.*, on geology of Texas and Chihuahua, viii, 378.
Silver mines of Chihuahua, ix, 161.
King, *C.*, fossils collected by geol. survey under, l, 422.
King, on Eozoön, iv, 375.
Kingsley, *J. L.*, on Weston meteor, vii, 1.
Kinnan, G., Tent life in Siberia, noticed, l, 439.
Kingston, *G. T.*, on aurora at Toronto, viii, 65.
meteoric astronomy, noitced, iv, 428.
Kirkwood, *D.*, comets of 1812 and 1846, viii, 255.
meteor of July, 1867, iv, 288.
meteorite of July, 1846, i, 347.
periods of meteoric rings, ix, 429.
Kirkwood, *J. P.*, on filtration of river-waters for city supply, viii, 446.
Knapp, theory of Bunsen flame, l, 255.
Knop. *A.*, kaolinite, iii, 405.
Knowlton, *W. J.*, new mineral from Rockport, Mass., iv, 224.
Kock, on system in botany, ii, 132.
Kohlrausch, *F.*, on influence of temperature on elasticity, l, 350.
Kokscharow's Mineralogie Russlands, noticed, i, 286; ii, 125; v, 121.
Kölliker's Icones Histiologicæ, ii, 283.
on polymorphism of Anthozoa, and structure of Tubiporæ, vi, 273.
Alcyonariæ, etc., noticed, l, 430.
Koschkull, *F. v.*, on the Caucasus, vi, 214. 335.
Kotschy, T., obituary of, v, 124.
Krœmer, on secondary chloral products, l, 109.
Krantz's catalogue, noticed, vi, 287.
Sœmann's minerals bought by, ii, 433.
Kundt, *A.*, tones in organ pipes, iii, 252.
velocity of sound, ii, 258.
Kurr, *J. G.*, Mineralogy of, noticed, ix, 119.
Kustel on ores, noticed, *Brush*, vi, 287, 432.
on gold and silver ores, noticed, l, 295.

L

Laboratory, The, noticed, iv, 144; v, 117.
Labradorite or norite rock, *Hunt*, ix, 180, 398.
Ladd, *W.*, dynamo-magnetic machine, v, 115.
Ladenburg, on silico-propionic acid, l, 416.
Lake, borax, of California, i, 255.
Lake-depressions, origin of, *Lesley*, i, 141.
Lakes, great American, flow of, vii, 145.
of N. A. as chronometers, noticed, *Andrews*, l, 264.
of N. A., sources of, *Shufeldt*, iii, 193.
Lalande prize, awarded to Watson, l, 293.
Land of the globe, maps of northernmost, noticed, iv, 883.
Land-slide, *Perkins*, ix, 158.
Lane, *J. H.*, theoretical temp. of Sun, l, 57.
Lang's Astron. tables, noticed, vi, 275.
Lapham, destruction of forest-trees, iv, 424.
geol. map of Wisconsin, vii, 279.
Lartet's Archæology of Southern France, noticed, iv, 119.
formation of Dead Sea, ii, 266.

and *Christy*, Reliquiæ Aquitanicæ, noticed, ii, 291; vi, 287; vii, 151, 279; viii, 135; ix, 144; l, 152.
Laurent, Traité d'algèbre, v, 75.
Laurentian and Huronian of Nova Scotia, etc., ix, 347; l 132, 417.
of Canada, fossils from, *Dawson*, iv, 367; graphite in, l, 130.
Lawson's discourse on Oxford Botanists, l 426.
rocks in Mass., ix, 75.
Laurite, with the platinum of Oregon, viii, 441.
Lavoisier, publication of works of, i, 105.
Lawrence Scientific School, Laboratory contrib., iv, 207, 224; v, 173; vii, 178, 319; l, 240.
Lea, I., Index to Unionidæ, noticed, v, 129.
new species of genus Unio, etc., by, viii, 144.
on Unionidæ, noticed, iii, 411.
synopsis of the family Unionidæ, l, 284.
Unios sensitive to light, vii, 430.
Lea, M. C., a theory of photo-chemistry, iv, 71.
detection of iodine, ii, 109.
germination, iii, 197.
light on iodid of silver, ii, 198.
new test for hyposulphites, iv, 222.
nitro-glucose, v, 381.
transmitted and diffused light, vii, 364.
Lea and Wilson's photographic mosaics, noticed, i, 143.
Lead, perchlorid of, *Nicklès*, iii, 94.
and bismuth, separation of by bromo-thallates, i, 107.
Leaves, absorption of moisture by, l, 428.
Le Conte, John, Cretaceous coal in New Mexico, v, 136.
Le Conte, Joseph, binocular vision, vii, 68, 153.
Le Conte J. L., rhynchophorous Coleoptera, iv, 41.
Leeds, A. R., form of aspirator, v, 423.
writing on a screen, v, 424.
Leffmann, H., ammonium-amalgam, ii, 72.
Lehmann on sugar in the blood, vi, 380.
Leidy, J., Elasmosaurus platyurus of Cope, ix, 392.
extinct mammalian fauna of Dakota, etc., noticed, ix, 274.
Megalonyx in Illinois, l, 270.
on Discosaurus and its allies, l, 139.
Sivatherium in Colorado, l, 270.
Leipzig, Astron. Gesellsch. publications, vi, 288.
Le Maout and Decaisne, Traité de botan., v, 409.
Lenz, properties of precipitated iron, l, 110.
Lereboullet, obituary of, i, 110.
Lesley, J. P., asphalt in Virginia, ii, 120.
petroleum at Brady's Bend, i, 123.
position of oil-wells, i, 139.
Lesleyite, *Smith*, viii, 254.
Lesquereux, L., Catalogue of mosses, noticed, v, 273.
Nebraska Cretaceous plants, vi, 91.
on fucoids in coal, noticed, ii, 264.
Level, change of, along Florida, i, 406.
Le Verrier on November meteors, iii, 299.
Levette, G. M., Indiana geol. survey, l, 135.
Levison, W. G., determination of magnesium group as oxalates, l, 240.
Lewis, J., shells planted in Erie canal, v, 137.
Lieben, normal amylic alcohol, l, 416.
Life, origin of, *Dana*, i, 389.
in hot waters, *Brewer*, i, 391; iv, 152; vi, 31.
tenacity of, in spores and seeds, i, 393.
Light, action on sulphurous acid, ix, 368.
amount of transmitted by crown glass plates, *Rood*, l, 1.
calcium, magnesium, etc., in photomicrography, ix, 295; l, 366.
chemical reactions caused by, *Tyndall*, vii, 129.
from gas, see *Gas*.
liquids of high dispersive power, l, 50.
magnesium, i, 106.
mechanical equivalent of, i, 214, 396.
new, for photography, i, 427.
on iodid of silver, *Lea*, ii, 198.
polarization of, *Chase*, ii, 111.
reflexion of, from glass, *Rood*, iii, 104.
spectral lines, *Hinrichs*, ii, 350.
transmitted and diffused, *Lea*, vii, 364.
visibility of neutral points, ii, 112.
wave-lengths, measurement of, l, 45.
of Fraunhofer's lines, i, 395.
see further *Star*, *Photometric*.
Lighting power for buoys, ix, 284.
Lime and magnesia salts, *Hunt*, ii, 49.
Linnean Soc., Bentham's addresses to, i, 410; iv, 297; l, 279, 325.
journal of, vi, 271; l 426.
Lippich, breadth of spectral lines, l, 106
Lippincott's vapor index, iv, 139.
Liquids of high dispersive power, l, 50.
Littrow, v. Carl, Sternschnuppen und Komuten, noticed, iv, 429.
Liver, sugar in, vi, 379; vii, 20, 258, 393.
Livingstone's African explorations, ix, 14.
safety of, v, 14, 141.
Lockyer, J. N., Astronomy by, vi, 275.
gaseous spectra, viii, 402.
spectroscopic examination of sun, viii, 121, 403.
spectrum analysis, ix, 432.

Loew, O., action of water on carbohydrates, iii, 371.
ferrocyanid of potassium on monochloracetic ether, v, 383.
nitrite of ammonia, vi, 29.
bisulphid of carbon in sunlight, vi, 363.
derivatives of tri-chlormethyl-sulphon-chlorid, vii, 350.
action of sunlight on sulphurous acid, ix, 368.
ozone from rapid combustion, ix, 369.
on hydrogenium-amalgam, l, 99.
Loewy, B., solar physics, iii, 179, 322.
Logan, W. E., Canada Survey, iii, 284.
Eozoön Canadense, vi, 245, 436.
Geological Atlas by, noticed, i, 408.
Map of Canada by, ix, 394.
Long, T., on Arctic explorations, vii, 105.
Longitude measured by telegraph, iii, 130.
trans-Atlantic, *Gould*, ix, 228.
Loomis, E., magnetic declination, auroras and sunspots compared, l, 153.
meteorology by, noticed, vi, 149.
on meteor of May 20, 1869, viii, 145.
Loomis, F. E., influence of temperature on elasticity, l, 350.
Lossen, Hydroxylamine, i, 251; ix, 254.
Loughlin, J. E., fluorescence, iii, 239.
molybdenum and chromium, vi, 131.
Lovén, S., on a recent Cystidean, viii, 429.
Lunar radiation, ix, 433.
see *Moon*.
Lutken on Ophiuridea, noticed, viii, 430.
Lyceum Nat. Hist. N. Y., Annals of, i, 288; ii, 292; iv, 296; v, 428; vii, 296; ix, 288, 444.
Lyell's Handbook of Ferns, noticed, ix, 404.
Lyman, C. S., Cambridge Physics, noticed, vi, 285.
new wave apparatus, v, 384.
observations of Venus, iii, 129.
Lyman, F. S., eruption in Hawaii, vi, 109.
Lyman, P. W., union of two trees, iii, 275.

M

Maack, G. A., Die bis jetzt bekannten Schildkröten, etc., noticed, l, 136.
Machinery, strength of, by Van Buren, noticed, viii, 151.
Magnesia and lime salts, *Hunt*, ii, 49.
in hydraulic cements, i, 425.
Magnesium, chromites of, *Nichols*, vii, 16.
group, determination of, as oxalates, *Levison*, l, 240.
light, i, 106.
colors by, iii, 91.
Magnetic declination, U. S., *Schott*, i, 149.
auroras and sunspots compared, *Loomis*, l, 153.
Magnetic filings phantoms, *Nicklès*, v, 68.
observations in Maine, *Bache*, ii, 141.
Magnetism, connotations of, *Chase*, vi, 398.
of chemical compounds, vii, 128.
origin of terrestrial, *Nicklès*, i, 110.
secular variation of terrestrial, Raulin, noticed, iv, 143.
Magnetite in mica, viii, 360.
Magneto-electric machines, new, iii, 107, 386.
Magneto-electricity, see *Electricity*.
Magnus, absorptive power of aqueous vapor, iii, 246.
on emission, absorption, and reflection of heat, l, 105.
on heat, ix, 106, 107.
obituary of, ix, 442.
Maine, Hist. Soc., collection of, noticed, viii, 437.
magnetic observ. in, *Bache*, ii, 141.
Post-tertiary fossils of, ii, 426.
Maize, American, *Atwater*, viii, 352.
Malden's island, iv, 383.
Malmgren's Arctic annelids, noticed, ii, 284.
Mammals of Massachusetts, *Allen*, ix, 134.
Mammoth Cave of Kentucky, Norwood, noticed, l, 439.
Man, remains of, *Andrews*, v, 180; *Nicklès*, v, 72.
in Belgium, iii, 121, 260.
in California drift, vi, 407.
in Kent's cavern, Devon., iii, 372.
skull of, discovered in Cal., ii, 424; *Whitney*, iii, 265.
Mandon, G., obituary, v, 124.
Manganese, action of peroxyd of, on uric acid, *Wheeler*, iv, 110, 218.
estimation of, as pyrophosphate, *Gibbs*, iv, 216.
separation of nickel and cobalt from, *Henry*, vii, 130.
tungstate of, *Blake*, iii, 125.
Mann, H., obituary of, vii, 143.
Manners, R. H., obituary of, l, 150.
Manufacturer and Builder, the, noticed, vii, 295.
Map, hydrographic, of the department of the Seine, *Delesse*, noticed, iv, 143.
of Arctic, noticed, iv, 383.
Maps, for showing distribution of plants and animals, vii, 295.
Marasse, on Rhenish creosote, l, 117.
Marignac, niobium and tantalum, i, 111, 397.
tantalum compounds, iii, 108.
fluosalts of antimony and arsenic, iv, 102.
Markownikoff, on conversion of isobutyl alcohol, l, 114.
Marsh, O. C., Ohio sepulchral mound, ii, 1.
Mastodon at Cohoes, iii, 115.

Marsh, O. C., Delesse's map of Seine. iii, 135.
Kansas geological Reports, iii, 283.
new genus of fossil sponges, iv, 88.
contributions to the mineralogy of Nova Scotia, No. I, iv, 362.
Paleotrochis of Emmons, and "cone in cone." v, 217.
geol. map of U. S., vi, 140.
metamorphosis of Siredon, vi, 364, 436.
new species of fossil horse, vi. 374.
Brazil reptilian remains, vii, 390.
on new Mosasauroid reptiles from N. J., viii, 392.
new species of Protichnites, viii, 46.
on fossil serpent from N. J., viii, 397.
birds from the Cretaceous and Tertiary of U. S., ix, 205, 272.
gavial from N. J. Eocene, l, 97.
Rocky Mt. expedition, l, 292.
Marshall's physiology by Smith, vii, 151, 435.
Martin, W. A. P., notes on China, vii. 98.
Martius, C. F. P. v., Akademische Denkreden, v, 143.
herbarium of. l 279.
obituary of, *Brewer*, vii 288.
Mastodon, discovery of a, l, 422.
Matteucci. C. obituary of, vi. 285.
Matthew, G. F., on rocks of New Brunswick noticed ii. 124.
Matthiessen. expansion of metals and alloys by heat iv, 110.
expansion of water and mercury, iii, 254.
on narcotine. etc., l. 256.
death of, l, 437.
Maxwell, J. C., real-image, stereoscope v, 116.
Mayer, A. M., notes on physics by, viii. 151.
on method of measuring electric conductivities, l, 307.
Researches in Electro-magnetism, l, 195.
report of eclipse, viii, 436.
McDonald, M., apparatus for collecting and washing precipitates in test-tubes, iv, 188.
Mechanical equivalents, comparison of, *Chase*, l, 261.
polarity, *Chase*, i, 90.
Mechanics *Weisbach's*, noticed, viii, 449.
Meek, F. B., Bellinurus Danæ, iii, 257, 394.
corals from Nevada, v, 62.
Ethmophyllum and Archæocyathus, vi, 144.
genus Aviculopecten, v, 64.
Hudson River group, iii, 256.
notice of Stimpson on the Hydroblinæ, i, 270.
Meek, F. B, on Geinitz's views on the Paleozoic of S. E. Nebraska, iv, 170, 282, 327.
on Paleozoic Crinoidea, viii, 23.
punctate Syringothyris, iii, 407.
shell-structure of Spirifera, vi, 262, 408.
the genus Palæacis, *Haime*, iv, 419.
on Bellerophontidæ, noticed, ii, 126.
on Crinoids, noticed, l, 135.
check-lists of N. A. fossils, noticed, iii, 363.
Geol. of Mackenzie river, by, noticed, v, 418.
on genus Taxocrinus, noticed, i, 124.
on Spirifer cuspidatus, noticed, i, 409.
Meek and *Hayden*, Paleontology of upper Missouri, iii, 363.
Meek and *Worthen*, Ill. paleontology, iii, 113.
on a fossil scorpion, etc., from Ill., vi, 19.
Meissner, Neue Untersuchungen über den elektrisirten Sauerstoff, l, 151, 213.
Mellitic acid, iii, 388.
Mercury, expansion of. iii, 255.
vapor of, effect on plants, v, 71.
Metallurgical method of Whelpley and Storer, *Hunt*, iii, 305.
Metallurgy, Crookes's, noticed, ix, 144, 286.
Metals in organic radicals, *Berthelot*, ii, 256.
influence of temperature on modulus of elasticity of, l, 350.
Meteor near Charleston, *Shepard*, i, 276.
of May 20th, 1869, viii, 145, 146.
Meteors during eclipse, *Himes*, viii, 435.
See *Shooting Stars*.
Meteoric fire ball of July, 1846, *Kirkwood*, i, 347.
iron, large Australian mass, i, 426.
of Colorado, ii, 218, 250, 286.
Franklin co., ix, 331.
Greenland, *Shepard*, ii, 249.
Hungary. *Szabo*. ii, 432.
Mexico, *Shepard*, ii, 347.
Tennessee, *Shepard*, ii, 251.
Virginia, *Shepard*, ii, 250.
Meteoric irons, l, 293.
analysis of, *Smith*, ix, 332.
rings, periods of, *Kirkwood*. ix, 429.
Meteorite of Alabama, viii, 240; ix, 90.
Aumale, i. 426.
Cohahuila, N. Mexico, *Shepard*, iii, 384; *Smith*, vii, 383.
Colorado, *Smith*, iii, 66; *Jackson*, iii, 280.
fall of, in Georgia, *Smith*, l, 293, 339; *Willet*, l, 335.
flight of, in Ohio, *Smith*, ix, 139.

Meteorite from Georgia, *Shepard*, vi, 257.
Germany, *Geinitz*, vi, 284.
Kansas, *Mudge*, vi, 429.
Knyahinga, *Haidinger*, iv, 131.
Mexico, *Smith*, v, 77.
U. S., new, *Shepard*, vii, 230.
Wisconsin, *Smith*, vii, 271.
Weston, Conn., Dec., 1807, vii, 1.
Meteorites, carbonaceous matter of, vii, 130.
classification of, *Shepard*, iii, 22.
Daubrée on, ii, 124.
experiments relative to, by *A. Daubrée*, viii, 418.
light, heat, and sound, accompanying fall of, *Haidinger*, viii, 280.
origin of, *Graham*, iv, 109.
Sorby on, i, 136, 137.
Meteorological comparison, *Chase*, i, 158.
observations, reductions of, *DeForest*, iii, 316.
Météorologiques de Bruxelles, noticed, iv, 144.
Meteorology, Buchan's, noticed, iv, 434.
Butler's, noticed, l, 150.
Loomis's, noticed, vi, 49.
on correcting an error in tables of temperature, *DeForest*, i, 371.
Methyl, arsenate of, l, 14.
arsenite of, l, 19.
benzyl, i, 112.
Mettenius, G. H., obituary of, v, 123.
Meunier, Science et les Savants in 1866, noticed, v, 76.
Mexican scientific commission, iii, 98.
Mexico, Rémond's geology of, ii, 261.
Michaux Grove of oaks, l, 279.
Microscope, illumination of opake objects in, i, 283, 424.
mechanical finger for, i, 331; ix, 304.
artificial light for photography, ix, 294; l, 356.
Microscopic test-plate of Nobert, *Woodward*, vi, 352; viii, 169.
Miller, W. A., Chemistry noticed, iv, 295.
spectrum of a new star, ii, 389.
obituary, l, 437.
Miller, W. H., crystallization of graphitoidal silicon, iv, 108.
Mineral analysis, process in, *Clarke*, v, 173.
resources of U. S, Browne on, noticed, vi, 430.
Taylor on, noticed, vi, 431.
states and territories, *Raymond*, viii, 136.
waters, analysis of, *Thomas*, ii, 196.
Onondaga, *Goessmann*, ii, 211, 368.
unknown, in Haddam columbite, *Shepard*, l, 93.

Minerals—
Albite, iii, 227; Albertite, viii, 362; Altaite, v, 312; Amblystegite, l, 270; Ambrosite, l, 273; Anatase, ii, 272; Andesine, ii, 107; Aphthitalite, l, 274; Apophyllite, ii, 270; Aquacreptite, vi, 256; Arksutite, ii, 94; Asbestus, ix, 271; Atelestite, ix, 401.
Barnhardtite, v, 319; Biphosphammite, l, 274; Biotite, ii, 91; Bismuthine, l, 94; Bismutite, l, 94; Boulangerite, v, 320; Brochantite, v, 321.
Calaverite, v, 314; Cassiterite, ii, 271; Chlorite, iv, 201; Chlorite and Chloritoid, ii, 91, 108, 421; Chrysolite, i, 120; l, 35; Cinnabar, ii, 125; Columbite, ii, 248; l, 90; Columbium minerals, ix, 402; Cookeite, i, 246; Copper-glance, iii, 124; Corundophilite, i, 394; ii, 91, 92, 269, 421; iv, 258, 283; vi, 256; Corundum, ii, 90, 421; ix, 272; Cosalite, v, 319; Cotunnite, ii, 247; Cryolite, ii, 93; v, 141; vi, 400; Cryophyllite, crystallized, ii, 268; Cryophyllite, iii, 217; Cyrtolite, iv, 224.
Danaite, iii, 125; Danalite, ii, 73; Descloizite, viii, 137; Diamond, iv, 61, 119; viii, 441; Diaspore, ii, 90, 108, 268; vii, 319; l, 96; Dolomite, containing manganese, l, 37; Durangite, viii, 179.
Ekebergite, iii, 403; Emery, ii, 83, 421; Enargite, v, 34; vi, 201; Eulytite, ix, 401.
Feldspars, ii, 125; iv, 398; Fibrolite, ii, 272; Franklinite, viii, 138
Gay-Lussite, ii, 220, 221; Gibbsite, ix, 402; Gieseckite, ii, 270; Gmelinite, iv, 362; Gold, i, 120, 125; Grahamite, ii, 420; Grothite, iv, 258; Guanapite, l, 273; Guanoxalite, l, 273.
Hagemannite, ii, 246; Halloysite, ix, 402; Hercynite, viii, 356; Herschelite, l, 272; Hessite, v, 309; Hornblende, ii, 271; Hortonolite, viii, 171; Hübnerite, iii, 123, 125.
Ivigtite, vi, 400; vii, 133.
Jamesonite, v, 36; Jefferisite, i, 247.
Kaolinite, iii, 351, 405; Kerargyrite, iii, 124.
Laurite, ii, 422; viii, 441; Laxmannite, l, 270; Lavroffite, l, 272; Ledererite, iv, 362; Lepidomelane, iii, 222; Lesleyite, vii, 319; viii, 254; Leucite, iv, 406; ix, 335; Leucophane, iv, 405; Limonite, iv, 219.
Magnetite, viii, 360; Malacone, iii, 228; Maldonite, l, 272; Marcylite, i, 210;

MINERALS:—
Margarite, ii, 90, 107; iv, 283; Melinophane iv, 405; Melonite, v, 313; Micas, iv, 403: ix, 272, 401; Microlite ?, l, 93, 95; Monazite, ii, 420; Montanite, v, 317, 318; Moronolite, i, 212.
Nacrite, iii 67, 351; Namaqualite, l 271; Native lead, i, 254; Natroborocalcite, v, 120.
Œllacherite, iv, 256; Opal pseudomorph, l, 37; Ouvarovite, ii, 268; Oxammite, l 274.
Pachnolite, i, 119; ii, 93; iii, 271; Paracolumbite, ii, 269; Partzite, iii, 362; iv, 119; Pattersonite, vii, 320; Pencatite, ix, 402; Petalite, iv, 405; Petzite, v, 309; Pholerite, iii, 351, 405; Phosphammite, l, 274; Phosphorchromite, l, 271; Predazzite, ix, 402; Proustite, iii, 124.
Rahtite, i, 209; Redondite, l, 96; Rutile, ii, 422.
Samarskite, ix, 402; Scapolite, iv, 403; Scheeletine, i, 215; Selagite, (a rock) ix, 401: Sellaite, l, 273; Selwynite, l, 272; Serpentine, ii, 272; Silicoborocalcite, v, 117; Sillimanite, ii, 272; Spinel, ii, 271; Spodumene, ii, 248; Steinmark, iii, 351; Struvite, l, 272.
Talcosite, l, 272; Taltalite, iii, 407; Tantalum, ix, 402; Taylorite, l, 274; Tellurium, v. 306, 313; Tennantite, iii, 67; Tephroite, vi, 231; Tetradymite, v, 306, 316; Tetrahedrite, iii, 67, 125; v. 37. 320; Turgite, iv, 219; Turnerite, ii, 420.
Uwarowite, i, 216.
Vanadiolite, l, 271; Vermiculite, l, 96.
Wavellite, ix, 402; Warnerite, iv, 403; Whitneyite, v, 305; Willemite, vi, 230; Wilsonite, v, 47; Wolframite, l, 271; Wollangongite, viii, 85; Wulfenite, iii, 125.
Isomorphism of gadolinite, datolite and euclase, ix, 400.
from N. Jersey, *Roepper*, l, 35.
of Elba, ix, 402.
of Mr. Schoolcraft, for sale, viii, 442.
paragenesis of, *Reuss*, ii, 271.
Sæmann's collection of, ii, 433, 435.
Stone implements of, ii, 272.
Mineralogical contributions, *Shepard*, l, 90; v, *Rath*, l, 270.
nomenclature, *Dana*, iv, 145, 436.
curiosities of Paris Expositien, *Blake*, v, 194.
Mineralogy, Adam's Tableau of, ix, 119.
Dana's, 5th ed., vi, 132.
Kurr's, noticed, ix, 119.
objects and method of, *Hunt*, iii, 203.
Mineralogy of Nova Scotia, No. I, *Marsh*, iv, 462.
Mining, Phillips on, *Silliman*, vi, 134.
Kustel on, *Brush*, vi, 432.
Miquel's Prolusis Floræ Japonicæ, v, 403.
Mississippi valley, J. W. Foster on, noticed, viii, 150.
Mitchell, H., Gulf-stream soundings, iii, 69.
Mitchell, W., obituary of, vii, 434.
on assaying, new edition, vi, 434.
Mixter, W. G., willemite and tephroite, vi, 230.
Molecular Mechanics, *Bayma* on, vi, 167.
Physics, *Norton*, i, 61, 196; vi, 167.
Molecules, size of, *Thomson*, l, 258.
Molloy, G., Geology and Revelation by, noticed, l, 151, 440.
Mollusca classified on basis of cephalization, *Morse*, ii, 19.
Molybdenum, mode of obtaining, vi, 131.
Molybdic and phosphoric acids combined, vi, 397.
Monochromatic flame, *Nicklès*, iii, 92, 93.
Montana, yield of mines of, i, 125.
elephantine tooth and tusk in, i, 427.
Monthly means, on correcting, *De Forest*, ii, 154, 289.
Moon, constitution of, Hansen on, *Newcomb*, vi, 376.
crater Linné, iv, 130.
crater of, obscured, *Birt*, iii, 411.
inequalities in mean motion of, *Newcomb*, l, 183.
radiation of, ix, 433.
of heat from, *Rosse*, viii, 436.
Moore, fossils in mineral veins, l, 265.
Lias and oolite of Australia, l, 269.
Morphology of human hand, *Wilder*, iv, 44.
Morse, E. S., classification of mollusca, ii, 19.
on Brachiopods, ix, 103.
Brachiopoda, a division of the Annelida, l, 100.
Moseley, Henry, cause of descent of glaciers, l, 263.
Mossman, S., Origin of Seasons by, viii, 150.
Mound, sepulchral, in Ohio, *Marsh*, ii, 1.
Mount Hood, ascents of, *Brewer*, ii, 472; *Hines*, iii, 416.
Mudge, B. F., footmarks in Kansas, i, 174.
Kansas geol. Report, iii, 283.
meteorites, vi, 429.
Muir, T., thermal units, l, 149.
Mulder, G. J., Die Chemie der Austrockenden Oele, etc., noticed, iv, 438.
Müller, F., botanical works by, i, 415, 416.
Müller, J., anthers, iii, 126.
on nomenclature of, *Gray*, iii, 126.
Murchison, R. I., extracts from address to Roy. Geog. Soc., v, 14.

Murchison, R. I., Siluria by, noticed, v, 121.
Murphy, J. J., cause of glacial climate, ix, 115.
Murray, A., geographical distribution of mammals, noticed, iii, 410.
Muscular power, *Frankland*, ii, 393.
force, source of, *Douglas*, v, 110.
Musée Teyler, Catalogue of, noticed, i, 287; iii, 284.
Museum of Comparative Zoology, Bulletin of, noticed, viii, 451.
report noticed, iv, 141.
Mushrooms, esculent, *Curtis* on, ii, 129.
Music harmony in, *Poole*, iv, 1.
Musical ratios, *Poole*, v, 289.

N

Naphtha from California tar, iii, 242.
Rangoon petroleum, iii, 251.
Naphthalin, Vohl's test for, vi, 130.
Narcotine, and its decomposition products, l, 256.
National Academy of Sciences, i, 423; ii, 287, 431.
Nature, a new weekly, viii, 452; ix, 287.
Natural Selection, Wallace on, noticed, l, 142.
Naturaliste Canadien, noticed, vii, 296.
Naval Observatory, Report on the Eclipse, ix, 134.
Navassa, Gaussoin on, noticed, ii, 439.
Nebraska, geol. survey of, iv, 284.
Negro instruments, *Innes*, i, 140.
Neumann's apparatus for determining velocity of sound, ii, 417.
Neurine, v, 258.
Neutral point, Brewster's, *Chase*, iv, 70.
Newberry, J. S., China coal formation, ii, 151.
extinct N. A. floras, vi, 401.
Geol. of basin of great Lakes, ix, 111.
old water courses, ix, 267.
on Chinese coal fossils, iii, 284.
on Ohio geol. survey, ix, 400.
on the oil region of Indian creek, etc., Ky., i, 284.
Newcomb, S., Hansen on the Moon's constitution, vi, 376.
inequalities in Moon's mean motion, l, 183.
mode of observing transits of Venus, l, 74.
on observation of corona, etc., during a total eclipse, vii, 413.
New England, eastern, geol. of, *Hunt*, l, 83.
Newton, H. A., Astronomical abstracts, iv, 127, 287, 426.
Hamilton's Quaternions, ii, 438.
Newton, H. A., recent contributions to astro-meteorology, iii, 285.
Shooting Stars, i, 58, 192, 273; ii, 429; iii, 78, 276; iv, 426; v, 78, 225; vii, 118, 399; ix, 244.
Watson's Astronomy, vi, 145.
recent auroras, l, 146.
New York, notice of paleontology of, by Hall, iv, 142, 275.
Regents of Univ., 20th Rep., vi, 262.
Nichols, W. R., chromites of magnesium, vii, 16.
Nickel and cobalt, separation of, ii, 254.
manufacture, products of, ix, 365.
nitrite of, *Erdmann*, iii, 248.
Nicklès, J., bibliography, iii. 99.
correspondence of, i, 103; iii, 89; iv, 66.
obituary of, vii, 434.
Niles, W. H., Burlington limestone formations, ii, 95.
Niobium and its compounds, i, 111, 397; v, 393, 396.
Nitrification, *Johnson*, vii, 234.
Nitrites, monatomic, iii, 388.
of cobalt and nickel, iii, 248.
Nitrogen in peat, *Schultzenstein* on, ii, 132.
pentoxyd of, preparation of, l, 112.
Nitroglucose, *Lea*, v, 381.
Nitroprussids, formation of, viii, 406.
Nobert's test-plate, *Woodward*, vi, 352.
nineteen band, viii, 169.
review of Stodder on, *Sullivant*, vi. 347.
Nodal figures in organ-pipes, iii, 252.
Nomenclature, Mineralogical, *Dana*, iv, 145, 436.
Norite, *Hunt*, ix, 180, 398.
North Polar expedition, Swedish, viii, 227.
Norton, W. A., Astronomy noticed, iv, 295.
corona in solar eclipses, l, 250.
molecular physics, i, 61, 196; vi, 167; ix, 24.
Norwood, U. S., Mammoth Cave of Kentucky, noticed, l, 439.
Nova Scotia Institute, Proceedings of, i, 144.
Laurentian rocks of, l, 132.
mines and minerals of, ii, 123.
Novara, Reports on expedition of, i, 430.
Nugent's optics, noticed, vi, 436.

O

Obituary—
Arnott, G. A. W., vi, 273; vii, 140.
Bache, A. D., iii, 282.
Berg, O., v, 124.
Blunt, E., ii, 433.

OBITUARY—
Brande, W. T., i, 428.
Brewster, D., v, 284.
Bridges, T., i, 265.
Burkhardt, J., iii, 283.
Cassin, J., vii, 291.
Cotting, J. R., v, 141.
Cuming, H., i, 265.
Dana, S. L., v, 424.
Daubeny, C., v, 124, 272.
Day, Jeremiah, iv, 291.
Delessert, F., vii, 140.
Dewey, C., v, 122.
Encke, J. F., iii, 10.
Engel, C. G. F., v, 282.
Erdmann, A. J., ix, 144.
Erdmann, O. L., ix, 144.
Falconer, H., i, 264.
Faraday, M., iv, 293; v, 145; vi, 34, 180.
Featherstonhaugh, G. W., iii, 135.
Forbes, J. D., vii, 294.
Forchhammer, i, 284.
Gasparini, v, 124.
Gibbes, R. W., ii, 435.
Giraud, J. P., Jr., l, 293.
Goldschmidt, H., iii, 89.
Gould, A. A., ii, 434.
Graham, T., ix, 144.
Greville, R. K., ii, 277.
Halliday, A. H., l, 294.
Hamilton, W. R., ii. 293.
Harvey, W. H., ii, 129, 273.
Hayes, Ezekiel, iv, 139.
Hooker, W. J., i, 1, 265.
Hörnes, M., vii, 294.
Jones, G., ix, 284.
Junghühn, F. W., i, 263.
Jukes, J. B., viii, 296.
Kennicott, R., ii, 435.
Kotschy, T., v, 124.
Krüger, H., i, 263.
Lerebouillet, D. A., i, 110.
Lessing, C. F., i, 263.
Lindley, J., i, 265.
Magnus, G., ix, 442.
Mandon, G., v, 124.
Mann, H., vii, 143.
Manners, R. H., l, 150.
v. Martius, C. F. P., vii, 288.
Matteucci, vi, 285.
Mattheissen, A., l, 437.
Mettenius, G. H., v, 123.
Miller, W. A., l, 437.
Mitchell, W., vii, 434.
Montague, J. F. C., i, 267.
Nicklès, J., vii, 434.
Page, C. G., vi, 149; viii, 1.
Paxton, J., i, 264.
Peabody, G., viii, 441.
Pedersen, J., l, 436.
Pelouze, T. J., iv, 137.
OBITUARY—
Pickett, E. J., iv, 292.
Plücker, J., vi, 149.
Poeppig, E., vii, 142.
Porter, J. A., ii, 290.
Reeve, L., i, 283.
Richardson, J., i, 265.
Riddell, J. L., i, 141, 267.
Rogers, H. D., ii, 136.
Rosing, A., vi, 148.
Rosse, Lord, v, 142.
Sæmann, L., ii, 435.
Sars, ix, 144.
Sartwell, H. P., v, 121.
Schacht, H., i, 264.
Scheele, A., i, 264.
Schlechtendal, D. F. L., v, 124.
Schomburgh, R. H., i, 264.
Schott, H., i, 264.
Schultz, C. H., v, 272.
Shumard, B. F., viii, 294.
Silbermann, J. F., i, 103.
Strong, T., vii, 293.
Sturm, J. W., i 264.
Treviranus, L. C., i, 264.
Turczaninow, N., i, 263.
Uhler, W. M., i. 429.
Unger, F., ix, 410.
Ward, N. B., vi, 273; vii, 141.
Whewell, W., i, 428.
Observatory of Chicago, i, 140.
Ocean currents, *Croll*, l, 118.
life in depths of, vi, 409, 413; viii, 451; ix, 129, 415.
on the currents of, ix, 413.
temperature of, at depths, ix, 410.
Occultator, *Gibbes*, vii, 191.
description of, *Hill*, vi, 299.
Ohio, geol. survey of, viii, 417.
peat-bed underlying drift in, l, 54.
sepulchral mound in, *Marsh*, ii, 1.
Oil, drying of, *Mulder*, iv, 428.
illuminating, from California tar, *Silliman*, iii, 242.
see also *Petroleum*.
Olmsted's Astronomy, Snell's ed., ii, 139.
Onondaga springs, *Goessmann*, ii, 211,368.
Optical notices, *Gibbs*, l, 45.
See *Light*.
Ordway, J. M., Assmuss on distillation, v, 274.
Schützenberger's treatise on dyeing, iii, 421.
Organic substances, artificial formation of, *Williams*, vi, 327.
Organisms, living, observations and experiments on, in heated water, *Wyman*, iv, 152; l, 391. *Brewer*; vi, 31, *Wood*.
Ornithosauria, *Seeley*, noticed, l, 134.
Orton, J., Andes of Quito, v, 90.
and the Amazon, noticed, l, 294.
geol. of Andes of Ecuador, vii, 242.

Orton, J., observations on the Andes and Amazon, vi. 203
Orton. E., on Peat-bed beneath drift in Ohio, l, 54, 293.
Ostrich, acclimatization of, i, 109.
Owen, R., On Life and Species, vii, 33.
Cavern of Bruniquel, l, 429.
on the Dimorphodon. l, 424.
Equines of South America, l, 424.
Owen, Richard, rock-salt, New Iberia, La., ii, 120.
Oxalic acid, synthesis of, *Berthelot*, v, 249.
Oxydizing mixture of nitric acid and chlorate of potassium, *Storer*, viii, 190.
Oxygen, electrized, second research on, *Meissner*, abstract of, *Barker*, l, 213.
new method for, iii, 389.
Ozone, density of, *Soret*, iv. 108.
from rapid combustion, *Loew*, ix, 369; *Than*, l, 255.
researches on, *Meissner*, l, 151, 213.
prod. of in rapid combustion, l, 255.
thallous oxyd, test for, viii, 410.

P

Packard, A. S., Drift of Labrador, i, 30.
glaciers in White Mt. valleys, iii, 42.
Guide to study of Insects, noticed, vi, 274; ix, 285.
Amer. Entomology for 1869, l, 431.
Page, C. G., obituary of, vi, 149; viii, 1.
Paine, J. A., Jr., catalogue of plants by, noticed, i, 130.
Scolopendrium officinarum, ii, 281.
Palæacis, Haime, *Meek, Verrill*, iv, 419.
Paleontology, vegetable, by W. C. Schimper, noticed, viii, 272.
see also *Geology* and *Zoology*.
Palladium and hydrogen, *Graham* on, vii, 417.
Paraffin, products of oxyd of, *Gill* and *Mensel*, viii, 416.
Parasitism. Crustacean, *Verrill*, iv, 126.
Paris Exposition, Barnard's report on, ix, 175.
Parker, H. W., on carbolizing birds, l, 283.
Parker, J. D., earthquake in Kansas, v, 129.
Parkhurst, photo-mapper, ix, 37.
star-mapper, viii. 163.
Parks of Colorado, iv, 351.
Parlatore, essay on cotton, noticed, iii, 272.
on botanical method. iii, 273.
on Coniferæ, iii, 272.
Parry's report on Kansas Pacific R. R. route, vii, 112.
Paxton, J., obituary of, i, 264.
Peabody Acad. Nat. Sci., memoirs, vii, 436.
Peabody, G., gifts of, ii, 433; iii, 131, 414.
obituary of, viii, 442.
Peat-bed beneath drift in Ohio, *Orton*, l, 54.
Peat, Johnson on, noticed, ii, 439.
nitrogen in, *Schultzenstein*, ii, 132.
of Chatham Is., i, 123.
Peckham, S. F., California petroleum samples, iii, 345.
apparatus for analysis of petroleum, iv, 230.
distillation of hydrocarbons, vii, 9.
on bitumens and the formation of asphaltum, viii, 131.
on origin of Albertite, viii, 362.
Pedersen, J., obituary, l, 436.
Pelouze, T. J., obituary of, iv 137.
Pengelly, W., exploration of Kent's cavern, Devon., iii, 372.
Penrose, method of predicting occultations, etc., by, noticed, viii, 449.
Perfumery, art of, noticed, iv. 141.
Perkins, G. H., molluscan fauna of New Haven, ix, 276.
on a land-slide, ix, 158.
Perrey, A., on earthquakes, noticed, v, 268.
sale of library of. ii, 290.
Perry, J. B., geol. of W. Vermont, vii, 341.
Petermann, A., Gulf Stream, etc., noticed, l, 295.
Peters, C. H. F., asteroids discovered by, ii, 135; (111), l, 285; viii, 400.
elements of asteroid (85), i, 277; (109), ix, 277; (111), l, 432; (112), l, 409.
Petit, Traité d'Astronomie, iii, 99.
Petroleum, apparatus for analysis of, *Peckham*, iv, 230.
California, falsified samples of, *Peckham*, iii, 345; *Silliman*. iii, 245.
geol. relations of, *Andrews*, ii, 33.
in Canada, *Winchell*, i, 176.
Mexico, *Crowther*, vi, 147.
New S. Wales, *Clarke*, ii, 267.
Russia, ii, 272.
S. Kentucky and Tenn., *Safford*, ii, 104.
of Archangel and Zante, i, 427.
on Allegheny river, *Lesley*, ii, 123.
or oil wells, position of, *Lesley*, i, 139.
origin of, *Lesquereux* on, ii, 264.
region of, in Ky., *Newberry*, i, 284.
synthesis of, *Berthelot*, iii, 251.
uplift of W. Virginia, *Evans*, ii, 334.
see also *Hydrocarbons*.
Pfeil, F. S., ammonium-amalgam, ii, 72.
Phillips, J. A., geology of California gold fields, vii, 134.
on mining and metallurgy of gold and silver, noticed, *Silliman*, vi, 134.
Philological convention, American, vii, 287.

Phin, J., Chemical History of the Six days of Creation, l, 294.
Phosphate from W. Indies, *Shepard*, vii, 428.
Phosphoric acid, iii, 109.
in diaspore. *Shepard*, l, 96.
and molybdic acids, vi, 397.
Phosphorus. terchlorid of, action of alcohols on, iii, 389.
Photo-chemistry, a theory of, *Lea*, iv, 71.
Photograph of solar prominence, *Young*, l, 404.
Photography, a new light for, i, 427.
see also *Light* and *Photo-Micography*.
Photoheliograph, work done with, at Kew observatory, ix, 431.
Photo-mapper, *Parkhurst*, ix, 37.
Photometric experiments, *Rood*, ix, 145; l, 1.
Photo-micrography, calcium light in, *Woodward*, l, 366.
magnesium and electric lights in, *Woodward*, ii, 189; ix, 294.
Physician's problems, *Elam*, noticed, ix, 444.
Physics, molecular, etc., *Norton*, i, 61, 196; ix, 24.
notes on. *Mayer*, viii, 151.
Physiological chemistry, notices of papers in, *Barker*, vi, 233, 379; viii, 49.
Physiology, Marshall's, noticed, vii, 151, 435.
Pickering, E. C., forms of spectroscope, v, 301.
Solar eclipse, viii, 425.
Pickett, E. J., obituary of, iv, 292.
Pigeaux, on the Leporide, v, 127.
Pisani, F., Comptoir mineralogique of, iii, 115.
on corundophilite, i, 394.
taltalite, iii, 407.
Planets, discovery of, *Watson*, iv, 426.
new, *Peters*. viii, 400.
secular perturbations of, *Hall*, l, 370.
see also *Asteroids*.
Plants, assimilation of nitrogenous bodies by, *Johnson*, i, 27.
of California, dried, iv, 123.
see *Botany*.
Platinum compounds, *Schneider*, viii, 411; ix, 109.
Plücker, J., obituary of, vi, 149.
Poeppig, E., obituary of, vii, 142.
Poisons, Wormley's micro-chemistry of, noticed, iv, 140.
Polarity in animals, and polycephalism, *Clark*, ix, 69.
mechanical, *Chase*, i, 90.
Polarization, skylight, in Nebraska, v, 96.
Poncelet Traité des propriétés des figures, iii, 99.
Poole, H. W., harmony in music, iv, 1.
Poole, H. W., on musical ratios, v, 289.
Pope. F. L., Manual of Electric telegraph by, noticed, viii, 150.
Population, decreasing increase of, iii, 141.
Porter, J. A., obituary of, ii, 290.
Porter, S., vowel elements, ii, 167, 303.
Portland Soc. Nat. Hist., Proceedings, vii, 436.
Potassio-platinic oxy-sulpho-platino-stannate, viii, 412.
Pourtales, L. F., Crinoids, etc., from deep sea dredgings, noticed, viii, 451.
Gulf-stream fauna, vi, 409, 413.
Prairies, *A. Fendler* on, i, 154.
Precession-period in chronology, l, 172.
Precipitates. app. for collecting and washing, in test-tubes, *McDonald*, iv, 188.
gelatinous, *Chatard*, l, 247.
washing of, *Bunsen*, vii, 321.
Premium offered by the Netherland Soc., etc., ix, 284.
Prizes for discovery of comets, ix, 442.
Proceedings of Societies viii, 152, 298, 451.
Proctor, R. A., star-drift, ix, 436.
Protichnites, new species of, from Potsdam sandstone, *Marsh*, viii, 46.
Protozoa, *Clarke*, ii, 320.
Pumpelly, R., delta-plain of China, and changes of Yellow R., v, 219.
geol. observ. in China, etc., i, 145.
Geol. of China, etc., by, noticed, iii, 408.
Pynchon's Chemical Physics, noticed, ix, 443.

Q

Quaternions. Hamilton's elements of, ii, 438.
Quicksilver mine, Santa Barbara, v, 5.
Almaden, Spain, v, 9.

R

Radicals, organic, metals in, *Berthelot*, ii, 256.
Railroad between Atlantic and Pacific, iv, 381.
Rain-fall affected by the moon, *Chase*, vi, 281; *Hennessey*, vi, 283.
Rammelsberg, on position of thallium, l, 108.
Ramsay, Geol. of N. Wales, noticed, ii, 265.
Rand, T. D., ivigtite, vi, 400.
Ranking's Medical Abstract, noticed, vi, 286.
Rath, G. v., Mineralogical contributions, noticed, ix, 401; l, 270.
Rathke, properties of selenium, l, 111.

Raulin, V., secular variations of terrestrial magnetism, noticed, iv, 143.
Raupach, O. F., earthquake at St. Thomas, v, 134.
Rawlinson, H. C., changes in the Aral sea, iv, 133
Raymond, R. W., Report on mineral resources by, viii, 136.
Reale Comitato geologico d'Italia, ix, 401.
Redfield, J. H., Bahamas hurricane, 1866, vii, 116.
Reemelin on wine-making, vi, 432.
Reeve. L., obituary of, i. 283.
Reichardt, recovery of uranium, l, 113.
Reimann on aniline, ix, 142.
Reinhardt on Brazil bone-caves, vi, 264.
Reliquiæ Aquitanicæ, noticed, ii. 291; vi, 287; vii, 151, 279; viii, 135; ix, 144; l, 152.
Rémond, A., geol. of N. Mexico, ii, 161.
notes on S. American geology, iii, 114.
Repsold circle, *Abbe*, iii, 207. 309.
Reptiles, Mosasauroid of New Jersey, *Marsh* viii, 392.
serpent, from N. J., *Marsh*, viii, 397.
Reptilia, Batrachia, etc., of N. America, *Cope*, viii, 451; l, 150.
Reptilian remains, *Cope*, ii, 425; vi, 263. 415; viii, 278; ix 390; l, 140, 268.
Reuss, paragenesis of minerals, ii, 271.
Richardson, J., obituary of, i, 265.
Richthofen, v., China explorations, l, 149, 410.
system of volcanic rocks, noticed, v, 267.
Riddell, J. L., obituary of, i, 141, 267.
Rock-salt, in Louisiana, *Goessmann's* report, iii, 284.
Rocks, lifted and subsided, of America, l, 135.
Rocky Mt. expedition, *Marsh*, l, 292.
scenery, sun-pictures of, *Hayden*, noticed, l, 125.
Rocky Mts., explorations in, *Whitney*, ix, 398.
Marsh, l, 292.
supposed absence of drift from Pacific slope of, *Brown*, l, 318.
Rodgers, J., tides of Tahiti, i, 151.
Rodman, C. S., analyses of turgite, iv, 219.
Rœmer, spider from coal formation, ii, 123.
Rogers, W. A., Asteroid (109), ix, 141, 428.
personal equation in transit observations, vii, 297.
Rogers, W. B., Pres. of Inst. of Technology, i, 141.
Rogers, H. D., library and geological specimens of, iii, 419.
Rogers, H. D., obituary of, ii, 136.
Rolfe and Gillet's Cambridge Physics, vi, 285.
Handbook of Chemistry, vii, 294.
Rood, O. N., tint from mixing blue and yellow, i, 369.
thermo-electric currents by percussion, ii. 12.
on measuring average size of fine particles, iii, 104.
on the discharge of a Leyden jar with the induction coil, viii, 153.
photometric experiments, Part I, ix, 145; Part II, l, 1.
Roepper, W. T., minerals from N. J., l, 35.
Root, E. W., enargite from California, vi, 201.
Wilsonite, v, 47.
Roscoe's Chemistry, noticed, iii, 137.
Spectrum Analysis, noticed, ix, 389.
Rosing A., obituary of vi, 148.
Rosse, Lord. obituary of, v, 142.
Lunar Radiator, viii, 436.
Rowley, S., theory of vision, vi, 153, 430.
Royal Geographical Soc., extracts from Murchison's address, v, 14.
Society of London, ix, 442.
Rumford medal to A. Clark, ii. 136.
Russia, central observatory of, ii, 286.
petroleum in, ii, 272.
Russian America, geography of, iv, 379; *Blake*, v, 242.
American explorations, *Dall*, v, 96.

S

Sadtler, S. P., Fischer's salt, ix, 189.
Sæmann, L., obituary of, iii, 435.
Safely, R., on mastodon remains, ii, 426.
Safford, J. M., geology of Tennessee by, noticed, i, 409; viii, 416.
petroleum in S. Kentucky, ii, 104.
Tennessee geological Report, noticed. vi, 435.
Safford, T. H., in charge of Chicago observatory, i, 140.
Salisbury, R. A., Genera Plantarum, ii, 280.
Salmon, acclimatization and vitality of, i, 109.
Salt, chemistry of, *Goessmann*, ix, 78.
deposit of, in Louisiana, *Owen*, ii, 120; *Hilgard*, vii, 77.
Salt springs, Onondaga, *Goessmann*, ii, 211.
Salter, Lingula flags of Wales, i, 262.
Sars, Michael, Memoires des Crinoids vivants, of, noticed, viii, 142.
on Quaternary of Norway, i, 286.
obituary of, ix, 144.
fund for, ix, 283.
Sartwell, H. P., obituary of, v, 121.

Saussure's Nat. Hist. of Mexico and Antilles, noticed, ii, 285.
Scarlet, on a pure, *Dall*, l, 291.
Schiaparelli's theory of meteors, iii, 291.
Schlectendahl, obituary of, v, 124.
Schneider, new sulphur salts, ix, 108, 253.
platinum compounds, ix, 109.
Schoolcraft, sale of minerals, etc., of, viii, 440.
Schorlemmer, new hydrocarbons, iii, 108.
Schott, C. A., U. S. magnetic declination, i, 149.
Schrauf, A., determination of atomic weights by optical means, iv, 113.
Schultz, C. H., obituary of, v, 272.
Schwabe, on sun-spots, etc., iv, 287.
Schutzenberger on dyeing, noticed, iii, 421.
researches on platinum, l, 144.
Scorpion, fossil, *Meek* and *Worthen*, vi, 19.
Scudder, S. H., entomological correspondence of Harris, viii, 143.
mole-crickets, vi, 417.
N. American Orthopters, vii, 435.
on Neuroptera, iii, 411.
carboniferous insects, vi, 419.
Sea, see *Ocean*.
Seal, habits of, etc., noticed, l, 431.
Seasons origin of, *Mossman*, noticed, viii, 150.
Secchi's L'Unita delle Forze, noticed, iv, 143.
Seeley, H. G., Ornithopsis, ix, 393.
Ornithosauria, noticed, l, 134.
Index to fossil remains in the Woodwardian Museum, noticed, l, 134.
on Owen's Dimorphodon, l, 425.
Selenium, properties of, *Rathke*, l, 111.
Serret, Cours d'Algébre, iii, 99.
Shades and shadows, Warren's problems in, noticed, iv, 139.
Sharples, S. P., analysis of minerals, ii, 271.
chemical tables by, iii, 139.
minerals from Chester Co., Pa., vii, 319.
new salt of cæsium, vii, 178.
precipitation of antimonous sulphid from boiling solutions, l, 248.
Sharswood, W., on the discovery that Unios are sensitive to light, ix, 422.
Sheffield Laboratory contributions, i, 246; ii, 196; iii, 200, 351; iv, 219; v, 34; vi, 230, 240.
Shepard, C. U., Sr., rahtite, etc., i, 209.
meteor near Charleston, i, 276.
corundophylite and paracolumbite, ii, 91, 92, 269, 421.
mineral notices, ii, 246.
meteoric-iron, localities, ii, 249, 347.
classification of meteorites, iii, 22.
supposed tadpole nests, iii, 99.
Shepard, C. U., Sr., Cohahuila meteoric iron, iii, 384.
Corundophilite, Chester, vi, 256.
on aquacreptite, vi, 256.
meteorite from Georgia, vi, 257.
new U. S. meteorites, vii, 230
origin of S. Carolina phosphate formation, vii, 338.
new mineral phosphate from W. I., vii, 428.
mineralogical contribution, l, 90.
on ambrosite, l, 273.
on the Guanape Island guano and its minerals, l, 273.
Shepard, C. U., Jr., S. Carolina nodular phosphates, vii, 354.
Shimper, W. P., Vegetable physiology by, noticed, vii, 272.
Shooting Stars, 1865, Nov., *Newton*, i, 58, 273.
Aug., 1866, ii, 286, 429.
1866, Nov., *Newton*, iii, 78, 276, 413.
1867, May, iv, 129; July, *Kirkwood*, iv, 288; Aug., *Newton*, iv, 426; Nov., *Newton*, v, 78, 225.
Aug., 1868, *Newton*, vii, 287.
Nov. 1868, *Newton*, vii, 118, 399.
1869, Nov., ix, 244.
connection with comets, iv, 128.
fall of, June 19, 1866, *Haidinger*, iv, 131.
in the southern hemisphere, iv, 429.
Nov. orbit of, *Adams*, iv, 127.
Nov., 1866, spectra of, v, 279.
proportions as given by different observers, *Newton*, i, 192.
recent investigations of, *Newton*, iii, 285.
treatise on, by Kirkwood, noticed, iv, 428.
see also *Meteors*.
Shufeldt, G. A., sources of great Lakes, iii, 193.
Shumard, B. F., Catalogue of Paleozoic fossils, noticed, i, 124, 410.
paleozoic fossils, noticed, ii, 118.
collections of, viii, 294.
Cretaceous of Texas, by, noticed, ii, 123.
obituary of, viii, 294, 442.
Siemens, electric machine of, iii, 386.
Siberia, tent life in, etc., noticed, l, 438.
Silbermann, J. F., obituary of, i, 103.
Silicates, anhydrous, relation between proportions of alkalies and amount of silica, *Dana*, v, 109.
chemical formulas of, *Dana*, iv, 252, 398.
determination of protoxyd of iron in, *Cooke*, iv, 347.

Silicic acid ethers, *Friedel* and *Crafts*, iii, 155, 331.
Silicon, carbon replaced by, ii, 255.
combinations of, with alcohol-radicals, *Friedel* and *Crafts*, ix, 307.
graphitoidal, cryst. of, *Miller*, i, 108.
hydrid of, iv, 107.
iodid of, *Friedel*, vi, 398.
new compounds of, iv, 105.
oxychlorid of, vi, 125.
Silliman, B., Sr., Weston meteor, 1807, vii. 1.
Silliman B., on Arizona, i, 289.
Gay-Lussite, Nevada, ii, 220.
naphtha and illuminating oil from California tar, iii, 242.
note on petroleum analyzed by, iii, 245; *Peckham*, iii, 345.
Grass Valley gold-mining district, iv, 236.
peculiar mode of occurrence of gold and silver, v, 92.
mastodon in California, v, 378.
Phillips on gold-mining, noticed by, vi, 134.
American Assoc. meeting, vi, 275.
transformation of Siredon, vi, 421.
illuminating power of gas, mixed with air, viii, 40.
on Wollongongite, viii, 85.
relation of the light from gas to the volume consumed, ix, 17.
on flame-temperatures, ix, 339.
note on Mr. Stimpson's paper on Farmer's Theorem, l, 377.
on the determination of the Photometric power of a rich gas by dilution, etc., l, 379.
Silliman, J. M., spectroscopic examination of the Bessemer flame, l, 297.
Silk, solution of, i, 427.
Silver-mines of Chihuahua, ix, 161.
mining, Phillips on, noticed, vi, 134.
Silver, sulphuret of, *Blake*, iii, 125.
Silvering upon glass, iii, 252.
Siredon, metamorphosis of, *Marsh*, vi, 364, 436; *Silliman*, vi, 421.
Sismonda, Anthracite rocks of Alps, iii, 409.
Chart of N. W. Italy, iii, 115.
Sitka, scientific party for, iv, 291.
Sky, color of, *Tyndall*, viii, 258.
Skylight polarization in Nebraska, *Chase*, iv, 265.
Smith, F. H., experiments with fluid jets, v, 419.
resolution of sounding flames, v, 421, 422.
Smith, H. L., on a microscopic finger, i, 331.
note on illumination of opake objects under the microscope, i, 424.
Smith, H. L., new binocular eye-piece, v, 42.
on the spectroscopic examination of Diatomaceæ, viii, 83.
Smith, J. L., emery of Chester, Mass., ii, 83.
Colorado meteorites, iii, 66.
minerals from Kellogg mines, Ark., iii, 67.
meteoric iron, Mexico, v, 77.
meteorites, Wisconsin, vii, 271.
Cohahuila meteorite, vii, 383.
on desCloizite, viii, 137.
on lesleyite, viii, 254.
Alabama meteorite, ix, 90.
meteorite in Ohio, ix, 139.
Franklin Co. meteoric iron, and analyses of meteoric irons, etc., ix, 331.
on alkalies in leucite, ix, 335.
fall of meteorite in Georgia, l, 293.
meteorite of Stewart Co., Ga., analysis, l, 339.
on use of flame heat in the laboratory, etc., l, 341.
Smith, S. I., on new crustacea, viii, 118.
on crustacea of Brazil, viii, 388.
on American Crustacea, No. I, noticed, ix, 426.
Smithsonian contributions to paleontology, iii, 363.
Institute, history of, *Bentham*, iv, 305.
library, transfer of, *Henry*, v, 137.
report, 1864, noticed, i, 143; 1865, ii, 436; 1867, vii, 295; 1868, viii, 450.
Smyth, R. B., gold-fields, etc., of Victoria, noticed, ix, 263.
Snell's Olmsted's Astronomy, ii, 139.
Soap and candles, manuf. of, noticed, iv, 141.
Sodium-amalgamation, *Wurtz*, i, 216.
flame, iii, 91, 94.
use of, in explosive powders, vii, 431.
Soils, specific heat of, *Pfaundler*, iii, 393.
Solar atmosphere, yellow ray of, vii, 415.
corona in total solar eclipses, *Norton*, l, 250.
eclipse, on observing, *Newcomb*, vii, 413.
physics, *De La Rue*, etc., iii, 179, 322.
prominence, photograph of, *Young*, l, 404.
protuberances, vii, 127, 273.
spectrum, vii, 127, 273, 274, 417.
map of, *Gibbs*, iii, 1.
spots, vapor of water near, vii, 416.
See also *Sun*.
Sombrero, geol. of, *Julien*, noticed, ii, 439.
Sorby, on meteorites, i, 136, 137.
spectra of zirconium and uranium compounds, ix, 387.

Soret, density of ozone, iv, 108.
Sound, transmission of, affected by interior friction of the air, iii, 253.
velocity of, *Kundt*, ii, 258; *Neumann*, ii, 417.
waves, interference, apparatus for, ii, 417.
Spectra, influence of electro-negative elements on, *Dracon*, i. 250.
ultra-violet, on the, *Mascart*, viii, 404.
gaseous, researches on, *Frankland* and *Lockyer*, viii, 402.
Spectral lines, *Hinrichs*, ii, 350.
breadth of, *Lippich*, l, 106.
of the elements, wave-lengths of, *Gibbs*, vii. 194; *Thalen*, vii, 272.
Spectroscope, a new, *Zöllner*. ix, 58.
forms of, *Pickering*, v, 301.
detection of chlorine, bromine and iodine by, i, 112.
Spectroscopic examination of the Bessemer flame, *J. M. Silliman*, l, 297.
of the Diatomaceæ, viii, 83.
Spectrum analysis, *Lockyer*, ix, 432; *Roscoe*. ix. 389.
aqueous lines of, *Cooke*, i, 178.
artificial, vii, 418.
measurement of wave-lengths, *Gibbs*, v, 298.
of aqueous vapor, *Nicklès*, iii, 90.
of Aurora of April, 1869, viii, 404.
of electric brush, iii. 394.
of the stars, v, 393; vii, 274, 416, 417; ix, 58.
of shooting stars, *Browning*, v, 279.
of *T* Coronæ, ii, 389.
observations during eclipse, vii, 127.
solar, vii, 127, 273, 274, 417.
map of, *Gibbs*, iii, 1.
stellar *Secchi*, v, 393.
violet portions of, *Angström*, v, 250.
Sponges, animality of, *Clark*, ii, 320.
new genus from the lower Silurian, *Marsh*, iv, 88.
Spontaneous generation, *Child*, i, 381; *Dana*, i, 389; *Verrill*, i, 418, 420; *Nicklès*, iii, 97.
Städeler, on yolk of eggs, vi, 235.
Stanley's logarithmic tables, error in, vii, 287.
Star-drift, ix, 436.
Star-mapper, *Parkhurst*, viii, 163.
Star, new variable, ii, 79, 80, 135.
spectrum of, *Huggins*. ii, 389.
photometer, results of, ii, 418.
Stars, fixed, D'Agelet's observations on, ii, 287.
Stars, heat of, *Huggins*, viii, 286; ix, 108.
heating power of, ix, 434.
spectra of. ix, 58.
Statistics, military and anthropological, *Gould*, viii, 149.
Steel, *Bessemer*, i, 278.
Stefan, sound affected by interior friction of air, iii, 253.
Stellar spectra, *Secchi*, v, 393.
Stereoscope, real image, *Maxwell*, v, 116.
Stevens, Henry, life of Humboldt, ix, 1.
on the earliest discoveries in America, viii, 299, 437.
Stewart, B., auroral appearances, etc., ix, 281.
solar physics, iii. 179, 322.
Stimpson on the Hydrobiinæ, noticed, i, 270.
Stimpson, F. E., discussion of Farmer's theorem, l, 372.
Stockwell, J. N., variation in elements of earth's orbit, vi, 87, 436; l. 147.
Stodder on Nobert's test-plate, review of, *Sullivant*, vi, 347.
Stone implements, Celtic, composition of, ii, 272.
Chatel's collection of, ii, 289.
of Elba, i, 427.
Stone, E. J., appointed astronomer at the Cape of Good Hope, l, 149.
Storer, D. H., fishes of Mass., noticed, v, 129.
Storer, F. H., hydrocarbons from animal fats, iii, 250.
Johnson's How crops Grow, noticed by, vii. 147.
naphtha from Rangoon petroleum, iii, 251.
on nitric acid and chlorate of potassium as an oxyizing mixture. viii, 190.
Storer and Eliot, chemistry of, noticed, iii, 420; vi, 130.
Storer, H. R., decreasing increase of population, iii, 141.
Storms, telegraph to announce. ix, 282.
Strecker, on sulph-acids, vi, 124.
on uric acid, vi, 124.
Strong, T., obituary of, vii, 293.
Struver, on sellaite, l, 273.
Subsidence of land at Coxsackie, i, 12.
Sugar in the liver, vi, 379; vii, 20, 258, 393.
test for distinguishing kinds, i. 427.
Sullivant, W. S., Musci Bor-Amer., by, i, 417.
notes on Stodder on Nobert's test-plate. vi, 347.
Sulph-acids, formation of, *Strecker*, vi, 124.
Sulphid, carbonylic, *Than*, v, 251; *Berthelot*, vi, 129; vii, 122.
Sulpho-carbamid, *Reynolds*, vii, 428.

Sulphur deposit, island of Saba, viii, 278.
in organic compounds, method of determining, *Warren*, i, 40.
new acid of, viii, 408.
salts, new, *Schneider*, ix, 108. 253.
Sulphuric acid of Gasteropods, ix, 420.
Suiphuric anhydrid, modifications of, l, 114.
Sulphurous acid, action of sunlight on, *Loew*, ix, 368.
Summer, Indian, *Willet*, iv, 340.
Sun, eclipse of, ix, 134.
of August 7, 1869, viii, 286; *Gould*, viii, 434; *Mayer's* report of, viii, 434; meteors during, *Himes*, viii, 435; *Pickering's* party, viii, 435; *Winlock*, viii, 434; *Young*, viii, 370, 453.
Foucault's objectives for observing, iii, 254.
light, action of, on glass, *Gaffield*, iv, 244, 316.
limb of, method of observing contacts at, *Young*, viii, 370.
method of viewing solar prominences without an eclipse, viii, 401.
physical constitution of, *Lockyer*, viii, 402, 403.
protuberances of, *Zöllner*, viii, 401.
spectroscopic examination of, *Lockyer*, viii, 121.
spots, *De la Rue*, etc., iii, 179, 322.
and auroras, compared with magnetic declination, *Loomis*, l, 153.
and other solar phenomena, *Schwabe*, iv. 287.
influenced by refraction, ii, 260.
theoretical temperature of, *Lane*, l, 57.
See also *Solar*.
Swallow, G. C., geol. report of Kansas, noticed, iii, 283.
notes on Kansas, i, 405.
Sweden, geol. map of, i, 429.
Szabo, meteorite in Hungary, ii, 432.

T

Tadpole nests, supposed, *Shepard*, iii, 99.
Tahiti, tides at, *Rodgers*, i, 151; *Winslow*, ii, 45.
Talbutt, J. H., new analytical processes, l, 244.
Tantalic acid in microlite, *Shepard*, l, 95.
Tantalum, *Marignac*, i, 397.
compounds, *Marignac*, iii, 108; *Deville* and *Troost*, v, 396.
reduction of, *Marignac*, v, 393.
Tapir, new species from Guatemala, *Gill*, l, 141.
young, *Verrill*, iv, 126.
Taylor on mineral resources of U. S., vi, 131.
Telegraph Co., explorations by, in Russian America, i, 139.
by sound, in Africa, i, 140.
longitude measured by, iii, 130.
Temperature, influence of, on modulus of elasticity of metals, *Kohlrausch* and *Loomis*, l, 350.
theoretical, of the sun, l, 57.
terrestrial, and solar spots, *Abbe*, l, 345.
of flame, ix, 339.
Tennessee, geology of, *Safford*, i, 409, vi, 435; viii, 416.
petroleum in, *Safford*, ii. 104.
Tenney's Nat. Hist. of animals, ii, 429.
Terbium, non-existence of, i, 400.
Terpin, native hydrated, *Blake*, iii, 202; *Johnson*, iii, 200.
Tests for parallelism in glass, *Gibbs*, l, 35.
Texas, geol. survey, Buckley's report, ii, 437.
Teyler Museum, Archives, noticed, vi, 286.
Thallic acid, *Carstanjen*, iv, 269.
Thallium and thallic acid, i, 106, 107.
ethyl compounds of, ix, 389.
position of among elements, l, 108.
salts of, *Strecker*, i, 114.
Thallous oxyd, a test for ozone, viii, 410.
Than on carbonylic sulphid, v, 251.
production of ozone in rapid combustion, l, 255.
Theory of existence, noticed, ix, 286.
Thermal units, *Meur*, l, 149.
Thermo-dynamics, St. Robert's, noticed, i, 287.
Thermo-electric currents by percussion, ii, 12.
Thioxyl, chlorid of, synthesis of, iii, 107.
Thomas, T. F., anal. of mineral water, ii, 196.
Thomson, W., size of atoms, l, 38.
Thomson, Wm., size of molecules, l, 258.
Tides at Tahiti, *Rodgers*, i, 151; *Winslow*, ii. 45.
Tillman's chemical nomenclature, iii, 140.
Tin, separation of from arsenic, etc., ix, 48.
Tin and tungsten, separation of, *Talbott*, l, 246.
Titanium, oxyd of, Chester, Mass., ii, 92.
Toluidine, new alkaloid isomeric with, viii, 123.
Tones, interferential, *Stefan*, iii, 253.
Töpler's method for finding differences of density, iii. 390.
Torrey, J., semi-centennial of, v, 273.
Traill's treatise on quartz and opal, ix, 403.
Transit observations, personal equation in, *Rogers*, vii. 297.
Transparency of the air, *Nickles* v, 70.
Treadwell, D., new cannon, i, 97.

Treadwell, D., strength of cannon, v, 135.
Trees, tall, in Australia, iv, 422; viii, 294.
Tri-chlor-methyl-sulphon chlorid, *Loew*, vii, 350.
Troost, heat of combination of boron and silicon with chlorine and oxygen, ix, 386.
Trowbridge, D., meteors, Aug. 1866, ii, 286.
Tungsten, chlorids of, *Debray*, ii, 254.
Turpentine, oil of, and camphor, hypochlorous acid on, *Wheeler*, v, 48.
Twisden, Introd. to mechanics, v, 426.
Tylor, A., on the Amiens gravel, vi, 302, 436.
Tyler, S. W., analyses of rahtite, marcylite, and moronolite, i, 209.
Tyndall, John, on chemical reactions caused by light, vii, 129.
on clouds, viii, 258.
on color of sky, polarization of skylight, etc., viii, 258.
on sound, noticed, v, 286.
sketch of Faraday, vi, 34, 180.

U

Ulrich, G. H. S., mineralogy of Victoria, l, 271.
Unio, new species of, *Isaac Lea*, viii, 144.
Uranium, action of light on, *Bolton*, viii, 206.
recovery of, from the phosphate, *Heintz. Reichardt*, l, 113.
Urea, formula of, *Heintz*, vi, 237.
Uric acid, *Gibbs*, vi, 289; *Strecker*, vi, 124.

V

Vanadium, *Roscoe*, viii, 407.
researches on, *Roscoe*, v, 394.
Van Beneden, commensalism among animals, l, 285.
Van Buren, J. D., machinery, strength of, by, viii, 151.
Van Nostrand's Engineering Magazine, vii, 295.
Vapor, adhesion of, *Gibbs*, iv, 101.
Vapor index, Lippincott's, iv, 139.
Vegetable, see *Botany*.
Venus, observations of, *Lyman*, iii, 129.
transit of, ix, 435.
mode of observing, *Newcomb*, l, 74.
Vermont, notes on geol. of, *Hunt*, vi, 222.
geol. of, *Perry*, vii, 341.
Verrill, A. E., notice of Wilder on morphology of limbs of mammals, i, 132.
distribution of North American birds, i, 249.
new fluid for preserving specimens, i, 268.
Verrill, A. E., notice of Clark's Mind in Nature, i, 418.
zoological notices, ii, 132, 283, 427; ix, 129, 273, 276, 423; l, 430.
polyps of Panama, noticed, ii, 428.
and corals of Pacific, noticed, ii, 428.
and echinoderms of New England, noticed, ii, 428.
crustacean parasitism, iv, 126.
on radiates in the museum of Yale College, and on some new species, iv, 125.
Echinoderms and Corals of Panama and Western America, iv, 125.
young of the Central American tapir, iv, 126.
Halcyonoid polyps in Yale College museum, v, 411.
on Hartt's corals and echinoderms from Brazil, v, 416.
on echinoderms from Lower California, v, 417.
Alcyonaria in Yale College museum, vi, 143.
notice of Hyatt on the Polyzoa, vi, 150.
on Radiata in Yale College museum, noticed, vii, 286.
on zoological nomenclature, viii, 92.
halcyonoid polyps, vii, 282; viii, 419.
on new jelly-fish and actineans, viii, 116.
phyllopod crustacea, viii, 244.
on localities of Artemia, viii, 430.
on corals and polyps of Exploring Expedition, viii, 431.
of West coast of America, viii, 432.
echinoderms and corals from Gulf of Cal., ix, 93.
sea-urchin of New England, ix, 101.
deep sea faunæ from recent dredgings, ix, 129.
shells of Gulf of California, ix, 217.
new corals, ix, 370.
new species of Entozoa, l, 223.
New England Nudibranchiata, l, 405.
parasites of man, l, 430.
Vieille, Eléments de Méchanique, iii, 99.
Vilmorin-Andrieux and Cie, Fleurs de pleine terre, v, 269.
Vision, binocular, *Le Conte*, vii, 68, 153.
theory of, *Rowley*, vi, 153, 430.
Vohl's test for naphthalin, vi, 130.
Volcanic action, seat of, *Hunt*, l, 21.
crater of Maui, ix, 43.
eruption at Santorin, i, 403.
in Hawaii, i, 424; iii, 264; vi, 105; vii, 89; ix, 269.
Leon, Nicaragua, v, 131.
Volanoes, works on, for sale, ii, 290.

Vose's Orographic geology, noticed, ii, 123, 205.
Vowel elements in speech, *Porter*, ii, 167, 303.

W

Wachsmuth, C., Burlington limestone formations, ii, 95.
Wales, North, geol. of, *Ramsay*, on, ii, 265.
Walker prize subjects, Bost. Soc. Nat. Hist., ii, 136.
Wallace, A. R., on Natural Selection, noticed, l, 142.
Walz, J., oxydation of diamylene, v, 57.
Ward, H. A., casts of fossils by, ii, 136.
Ward, N. B., obituary of, vi, 273; *Gray*, vii, 141.
Warren, C. M., chlorine in organic compounds, determ. of, ii, 156.
hydrocarbons from animal fats, iii, 250.
in Pennsylv. petroleum, v, 262.
naphtha from Rangoon petroleum, iii, 251.
sulphur in organic compounds, determination of, i, 40.
Warren, S. E., geom. problems by, noticed, iii, 284.
Shades and Shadows, noticed, iv, 139.
Washing of precipitates, *Bunsen*, vii, 321.
Washington, Mt., height of, iv, 377; v, 424.
Water, absorptive power of vapor of, ii, 259.
expansion of, iii, 254, 393.
from conduit pipe, delivery of, v, 191.
of Lake Leman, color of, *Hayes*, ix, 186.
on filtration of, by *Kirkwood*, noticed, viii, 446.
vapor of, absorptive power of, iii, 246.
in stars, *Nicklès*, v, 70.
spectrum of, iii, 90.
Waters, mineral, see *mineral*.
Watson, J. C., discovery of planets, iv, 426; vi, 274, 392.
Theoret. Astronomy by, noticed, vi, 145.
receives the Lalande prize, in astronomy. l, 293.
Watson's index to Indian names, vii, 143.
Wave-apparatus, *Lyman*, v, 384.
Wave-lengths, measurement of, by refractive indices, *Gibbs*, l, 45.
Weil, volumetric estimation of copper, l, 108.
Weisbach's mechanics, noticed, viii, 449; ix, 144.
Weld, M. C., remains of beaver in N. J., viii, 443.
West Virginia, oil-bearing uplift of, *Evans*, ii, 334.
Wetherill, C. M., crystalline nature of glass, i, 16.
on itacolumite, iv, 61.
Wharton, J., autumnal foliage, vii, 251.
products of nickel manufacture, ix, 365.
Wheatstone's electric machine, iii, 386.
Wheeler, C. G., action of peroxyd of manganese on uric acid, iv, 218.
hypochlorous acid on oil of turpentine and camphor, v, 48.
on a process of elementary analysis, i, 33.
Whewell, W., obituary of, i, 428.
Whelpley and Storer's metallurgical method, iii, 305.
Whitaker, W., subaërial denudation, vi, 268.
White, C. A., appointed geologist of Iowa, ii, 272.
drift in S. W. Iowa, iii, 301.
geol. of Iowa, iv, 23; l, 136.
Iowa coal measures, v, 331.
coal in Nebraska, v, 399.
Iowa geol. report by, noticed, iii, 284; v, 402.
shell-structure of Naiades, v, 400.
on "Cone in cone," v, 401.
Unios and light, vii, 280.
White Mts., glaciers in, *Packard*, iii, 42.
Whitney, J. D., geol. report of California, noticed, i, 124, 231, 351.
borax in California, i, 255.
new facts in geol. of California, i, 252.
human skull from shaft in Cal., iii, 265.
Nevada Silurian, iii, 267.
publications of Cal. geol. survey, viii, 133, 148, 151.
Yosemite guide-book, by, noticed, viii, 148.
explorations in Rocky Mts., ix, 398.
Whitney, H. M., eruptions in Hawaii, vi, 112.
Whittlesey, C., explorations in Minnesota, noticed, ii, 440.
Wiedemann, on magnetism of compounds, vii, 128.
Wilder, B. G., on morphology in limbs of mammals, i, 132.
morphology of the hand, iv, 44.
notice of Jour. of Anat. and Physiol., v, 124.
on extra digits, noticed, vii, 150.
Willet, J. E., fall of meteorite in Georgia, l, 335.
Williams, C. G., artificial formation of organic substances, vi, 327.

Williams, S. W., hail in China, iii, 281.
Will's qualitative analysis tables, noticed, iv. 140.
Wimmer, Salices Europeæ. iii, 272.
Winchell, A., petroleum in Canada, i, 176.
fossils of Marshall group, by, noticed, i, 120.
Niagara limestone fossils near Chicago, by, noticed, i, 409.
on Grand Traverse region, noticed, ii, 268.
geological chart by, vi, 140.
Sketches of Creation by, noticed, ix, 400.
Winds, motion of dome at Washington by, ix, 384.
Wine-making, Remelin on, noticed, vi, 432.
Wing, C. H., on double sulphates, ix, 356.
Winkler's Musée Teyler catalogue, iii, 284; v, 121.
Winlock, J., on solar eclipse, viii, 434.
Winnecke, discovery of comet, l, 104.
Winslow, C. F., tides and earthquakes, ii, 45.
human remains in California, vi. 407.
Wöhler, F., graphitoidal boron, iii, 250, 388.
laurite and diamonds in Oregon. viii, 441.
Wolf, C., equivalent of cerium, vi. 53.
Wolfram, transparent, *Des Cloizeaux*, viii, 137.
Wolle, C. F., on supposed hercynite, viii, 350.
Wood, H. C., Algæ from California hot spring, vi, 31.
Woods, N. A., Myriapoda, noticed. i, 135
Woodward, H., new Paleozoic fossils, ii, 264, 272.
Woodward, J. J., photo-micrography, ii, 189.
Nobert's test-plate, vi, 352.
nineteen band test-plate, viii, 169.
magnesium and electric lights in photo-micrography, ix, 294.
Calcium light in photo-micrography, l, 366.
Workshop, The, noticed, vi, 151, 434.
Wormley's Micro-Chemistry of Poisons, noticed, iv, 140.
Worthen, A. H., geol. survey of Ill., ii, 291; iii, 110, 395; vii, 151.
Paleontology of Illinois, etc., iii, 113.
on paleozoic crinoidea, viii, 23.
Wright, A. W., form of discharge between poles of electric machine, ix, 381.
Wurtz, A., synthesis of aromatic acids, l, 115.
Wurtz, H., sodium amalgam, i, 216.
on grahamite, ii, 420.
atmospheric air mixed with gas, viii, 40.
gas-well in New York, ix, 336.
on flame temperatures, ix, 339.
Wyman, J., observations and experiments on living organisms in heated water, iv, 152.
on a crocodile in Florida, ix, 105.

Y

Yale College, Peabody's gift to. iii, 131.
Yosemite guide-book, viii, 148.
Young, C. A., printing chronograph, ii, 99.
on a new method of observing contacts at the sun's limb, etc., viii, 370, 453.
photograph of solar prominence, l, 404.
Yttria, *Bahr* and *Bunsen*, i, 399.

Z

Zinc and manganese precipitated as sulphids. *Talbott*, l, 244.
Zirconium, new element associated with, viii, 405.
Zöllner, J. C. F., on astro-photometer, ii, 418.
observations on the protuberances of the sun. viii, 401.
new spectroscope, etc., ix, 58.
Zoological nomenclature, *Verrill*, viii, 92.
Zoological Works, noticed—
Allen, H., Comparative Anatomy, vii, 149.
Allen, J. A., Catalogue of Mass. reptiles and batrachians, vii, 285.
Bischoff, on gorilla, chimpansé, and orang-outang, iv, 142.
Clark, H. J., Spongiæ ciliatæ, v, 418.
Cope, E. D., Families of raniform Anura, v, 418.
Allegheny fishes, Va., vii. 285.
Batrachia, Reptilia, and Aves, extinct, of N. A., by, viii, 451.
Coues, Colymbus torquatus, iii, 411.
Birds of N. England, vii, 150.
S. Carolina, vii, 150.
Edwards, N. American butterflies, vi, 150, 436; vii, 150.
Gray, Synopsis of the star-fishes, in the British Museum, iv, 426.
Grote, Zygænidæ of Cuba, iii, 411.
Hyatt, molluscous order Tetrabranchiata, iv, 124.
Journal of Anat. and Physiol., v. 124.
Polyzoa, vi, 150.

ZOOLOGICAL WORKS, noticed:—
Lea, Unionidæ, iii, 411.
Index Unionidæ, v. 129.
Murray, geographical distribution of mammals, iii, 410.
Packard, Invertebrate fauna of Labrador, iv, 117.
Study of Insects, vi, 274.
Parker and *Jones*, oceanic Foraminifera, i, 287.
Record of Zoological literature, 1864, i, 287; 1865, iv, 144.
Scudder, Fossil Neuroptera, iii, 411.
N. Amer. Orthoptera, vii, 435.
Storer, Fishes of Mass., v, 129.
Stimpson on the Hydrobiinæ, i, 270.
Verrill, Polyps and Corals of N. Pacific Expl. Exped., i, 136.
on Hartt's corals and echinoderms from Brazil, v, 416.
Echinoderms from Lower Cal., v, 417.
Radiata in Museum of Yale College, iv, 125; vii, 286.
Wilder, morphology and teleology in limbs of mammals, i, 132.
Wood, myriapoda, i, 135.
ZOOLOGY—
Alcyonaria, in Y. C. Museum, *Verrill*, vi, 143.
Animal Commensalism, *Van Beneden*, l, 285.
Anthozoa, polymorphism of, vi, 273.
Birds of N. America, distribution and migration of, i, 78, 184, 249, 337.
Elliott's proposed work on, i, 288.
on carbolizing, *Parker*, l, 283.
Brachiopoda, a division of annelida, *Morse*, l, 100.
early stages of, *Morse*, ix, 103.
Butterflies of N. A., part 5, ix, 427.
Carboniferous insects, *Scudder*, vi. 419.
Corals and polyps of West Coast of America, *Verrill*, viii, 432.
Crustacea, new, Æthra, Scylaridæ, *Smith*, viii, 118.
of Brazil, *Smith*, viii, 388.
Phyllopod. viii, 240, 430.
American, ix, 426.
Cystidean, recent, *Lovén*, viii, 429.
Dimorphodon, l, 134, 424, 425.
Dodo, skeletons of, found, i, 273.
Echini, crinoids, etc., from deep sea dredgings, viii, 451.
ZOOLOGY—
Echinoderms and Corals from Gulf of Cal., *Verrill*, ix, 98.
Elasmognathus Dowii, *Gill*, l, 141.
Entozoa, new species of, *Verrill*, l, 223.
Ethmophyllum and Archeocyathus, *Meek*, vi, 144.
Flying fish, flight of, *Mann*, i, 272.
Fossil, see *Geology*.
Gasteropods, sulphuric acid of, ix, 420.
Gulf stream fauna, *Pourtales*, vi, 409, 413.
Halcyonoid polyps in Yale College Museum, *Verrill*, v, 411; vii, 282; viii, 419; *Kölliker*, l, 430.
Horse, fossil, *Marsh*, vi, 374; *Owen*, l, 424.
Jelly-fish and actinians from Maine, *Verrill*, viii, 116.
Læelaps, *Cope*, vi, 415.
Leporide, *Pigeaux*, v, 127.
Life and Species, *Owen*, vii, 33.
Living forms in hot waters of California, v. 239.
Mole crickets, *Scudder*, vi, 417.
Molluscan fauna of New Haven, ix, 276.
Natural Selection, *Wallace*, l, 142.
New Corals, *Verrill*, ix, 370.
Nudibranchiata, New England, *Verrill*, l, 405.
Pennatula family, *Richiardi*, ix, 426.
Polyps and crustacea of the Ringgold Exploring Expedition, *Verrill*, viii, 431.
Protozoa, i, 421.
Scorpion, etc., from Ill., *Meek* and *Worthen*, vi, 19.
Sea-urchin of New England, *Verrill*, ix, 101.
Shells planted in Erie Canal, v, 137.
of Gulf of California, *Verrill*, ix, 217.
Siredon metamorphosed into Amblystoma, *Marsh*, vi, 364, 436; *Silliman*, vi, 421.
Tapir, new species of, *Gill*, l, 141.
Tubiporæ, structure of, vi, 273.
Unionidae, Synopsis of, *Lea*, l, 284.
Unios sensitive to light, *White*, vii, 280; *Lea*, vii, 430.
Zoology, new preserving fluid for, *Verrill*, i, 269.